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ABSTRACT

Interim estimates were made of program impacts on earnings and employment of adults and out-of-school youths during the first 18 months after their acceptance into employment and training programs funded under Title IIA of the Job Training Partnership Act (JTPA). In the national study, 20,601 JTPA applicants in 16 service delivery areas were randomly assigned to treatment (allowed access to the program) or control groups. Analysis was based on a subsample of 17,026 participants interviewed 18 months after assignment. Different service strategies (classroom training, on-the-job training/job search assistance, and other services) were used for four subgroups: adult men, adult women, out-of-school male youths, and out-of-school female youths. Findings included the following: (1) Title IIA had generally positive effects on employment and earnings of adults in the treatment group; (2) it had little or no effect on average earnings of female youths, actually reduced earnings of male youths, and had no significant effect on youth employment rates; (3) almost all the negative impact on male youths' earnings was among those who had been arrested between age 16 and random assignment (25% of the subgroup); (4) earnings impacts reflected amount of time worked and how much was earned per hour; and (5) sites did not differ significantly in degree of effect on earnings. It was concluded that JTPA Title IIA is helping raise the earnings of many participants, especially adults. It is possible that adult/youth differences are the result of age alone. Ways to improve program effects for those groups negligibly or adversely affected by the program will require experimentation with a range of alternative service strategies and rigorous evaluation of their impacts. (Appendices contain the following: comparison of study sample with nationwide Title IIA participants; comparison of baseline characteristics of treatment and control groups; data sources; sample definition and impact evaluation method; comparison of data from first follow-up survey and state unemployment insurance agencies; analysis of nonenrollees; estimation of impacts on latent wage rates; and exhibits on out-of-school youth. There are 67 references and 156 data tables.) (YLB)

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A draft report to the
U.S. Department of Labor
Larry L. Orr, Project Director

The National JTPA Study

Title II-A Impacts on Earnings and Employment at 18 Months

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The National JTPA Study

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Executive Summary

THE National JTPA Study was commissioned by the Employment and Training Administration of the U.S. Department of Labor (DOL) in 1986 to measure the impacts and costs of selected employment and training programs funded under Title II-A of the Job Training Partnership Act of 1982, which is targeted to serve economically disadvantaged Americans. This report presents interim estimates of program impacts on the earnings and employment of adults and out-of-school youths in 16 local service delivery areas during the first 18 months after their acceptance into the program.

Estimates of longer term program impacts on earnings, employment, and welfare benefits, and an analysis of program costs and benefits, will appear in the final report of the study (forthcoming, from Abt Associates Inc.). A companion report on the study's implementation (Doolittle, forthcoming) describes the JTPA programs operated in the study sites and the types of JTPA-funded services provided to members of the study sample.

The National JTPA Study

This study grew out of the recommendations of the Job Training Longitudinal Survey Advisory Panel, a group of nationally recognized experts in employment and training research formed to advise DOL on the evaluation of JTPA (Stromsdorfer et al., 1985). After reviewing evaluations of Comprehensive Employment and Training Act (CETA) programs, the panel concluded that the only reliable way to measure the impacts of employment and training programs was to conduct a classical experiment, in which program applicants are randomly assigned either to a *treatment group*, which is allowed access to the program, or to a *control group*, which is not. Random assignment assures that the two groups do not differ systematically in any way except access to the program. Thus, subject only to the uncertainty associated with sampling error, any subsequent differences in outcomes between the two groups can confidently be attributed to the program. These differences are termed *program impacts*.

Although random assignment designs have been used to evaluate a number of demonstration projects and state programs, the Employment and Training Administration was the first federal agency to apply this approach to an ongoing national program. Because of its rigorous design, the National JTPA Study provides the first reliable estimates of the impacts of the largest employment and training program sponsored by the federal government.

In the National JTPA Study 20,601 JTPA applicants in 16 service delivery areas (SDAs) across the country were randomly assigned to the treatment group or the control group over the period November 1987 through September 1989. The earnings and employment outcomes of both groups were then measured through follow-up surveys and administrative records obtained from state unemployment insurance agencies. Data on the baseline characteristics of the two groups were collected as part of the program intake process, and information about the employment and training services received was obtained from follow-up surveys and SDA records.

The study sites were not chosen to be representative of the nation in a statistical sense, but they do reflect the diversity of local programs and local environments in JTPA. In particular, the performance of the sites during the study period, as measured by JTPA performance indicators, was not noticeably different from that of all SDAs nationally.[1]

The 18-Month Impact Analysis

This report provides estimates of the impact of JTPA Title II-A on the earnings and employment of four *target groups*—adult women and men (ages 22 and older) and female and male out-of-school youths (ages 16 to 21)—over the first 18 months after random assignment. Adult women make up 30 percent of the national JTPA population; adult men, 25 percent; and out-of-school youths, 23 percent. In-school youths, who are not included in this study, form the remaining 22 percent.

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[1] See Appendix B and Chapter 3 for comparisons of the 16 study sites with all SDAs nationally.

The analysis is based on a subsample of 17,026 sample members whose First Follow-up Survey interview was scheduled at least 18 months after random assignment.[2] For each target group we estimated impacts for a number of different subgroups, defined by the types of program services recommended for them and by their baseline characteristics.

Because the study was designed to measure the effects of JTPA as it normally operates, the analysis investigates which JTPA-funded services were working well for those recommended to receive them; the analysis does not assess possible alternatives to the existing program. By identifying those groups for whom Title II-A is having positive effects and those for whom it is having no effect—or even a negative effect—we hope to help policymakers in their efforts to identify those parts of the program that need improvement. This analysis cannot, however, tell policymakers *how* to improve the program, since it does not compare alternative programs for similar people. Rather, it measures only the effects of the existing program on the people it actually served over the study period.

In the remainder of this Executive Summary we first provide an overview of the estimated effects of the program on the earnings and employment of the four main target groups—adult women and men, and female and male youths. We then present more detailed findings for adult and youth subgroups, in turn, and conclude with implications of the findings for the JTPA program and future research.

Overall Impacts on Earnings and Employment, by Target Group

JTPA Title II-A had generally positive effects on the earnings and employment of adults in the study sites. As shown in the top panel of Exhibit S.1, access to the program increased the average 18-month earnings of the adult women randomly assigned to the treatment group ("JTPA assignees") by an estimated $539, or 7.2 percent of the control group mean. Access to the program also increased the percentage of women employed at some time during the follow-up period by 2.1 percentage points. Because these estimates are statistically significant (as indicated by the asterisks beside them), we take them to be reliable evidence of positive impacts on earnings. In this analysis we accept only statistically significant estimates as evidence of real program effects.

The estimated program impacts for adult men—an earnings gain of $550, or 4.5 percent, and an increase in the percentage employed of 2.8 percentage points—were similar in size to those for adult women, but the estimated impact on earnings was not statistically significant.

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[2] Within this 18-month study sample, First Follow-up Survey data are available for 14,442 sample members, or 84.8 percent of the sample.

Exhibit S.1 *Impacts on Total 18-Month Earnings and Employment: JTPA Assignees and Enrollees, by Target Group*

	Adults		*Out-of-school youths*	
Impact on:	*Women (1)*	*Men (2)*	*Female (3)*	*Male (4)*
		Per assignee		
Earnings				
In $	$ 539***	$ 550	$ -182	$ -854**
As a %	7.2%	4.5%	-2.9%	-7.9%
Percentage employed [a]	2.1**	2.8**	2.8	1.5
Sample size (assignees and control group)	*6,474*	*4,419*	*2,300*	*1,748*
		Per enrollee		
Earnings				
In $	$ 873[b]	$ 935[b]	$ -294[b]	$ -1,356[b]
As a %	12.2%	6.8%	-4.6%	-11.6%
Percentage employed[a]	3.5[b]	4.8[b]	4.5[b]	2.4[b]

[a] At any time during the follow-up period.

[b] Tests of statistical significance were not performed for impacts per enrollee.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

In contrast to the findings for adults, the program had little or no effect on the average earnings of female youths (a statistically *in*significant earnings loss of $-182, or -2.9 percent), and the program actually reduced the earnings of male youths, on average—as evidenced by a large, statistically significant loss of $-854, or -7.9 percent, over the 18-month period. Access to JTPA had no significant effect on the 18-month employment rates of either female or male youths.

Hence, the findings for the female youths are clear-cut: JTPA had virtually no effect on their earnings or employment. But the findings for male youths are less clear. As shown later in this summary, almost all of the negative average impact on the earnings of male youths is concentrated among those who reported having been arrested between age 16 and random assignment (25 percent of the male youth treatment group).[3] Thus, the estimated impact for most male youths (the 75 percent with no previous arrest) was negligible.

The estimates discussed above are average impacts on the earnings and employment of all sample members assigned to the treatment group. Although all of these assignees were given access to JTPA, not all of them actually enrolled in the program. The bottom panel of Exhibit S.1 presents our best estimates of program impacts on the earnings and employment of *JTPA enrollees* (assignees who were later enrolled).[4]

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[3] Furthermore, as noted later in this summary, there is some question about the large, negative impact estimated for male youths with a previous arrest.

Estimated impacts per enrollee—both gains and losses—were about 60 percent to 70 percent larger than impacts per assignee, depending on the target group. The estimated earnings gains of adult women and men who were enrolled in JTPA were $873 and $935, respectively. Impacts on youths were earnings losses of $-294 for females and $-1,356 for males.[5] The impact of the program on the percentage of enrollees in each target group who were employed ranged from an increase of 2.4 percentage points for male youths to an increase of 4.8 percentage points for adult men.

It is important to understand that the impact per assignee and the impact per enrollee are not two different estimates of the overall effect of the program. They simply spread the total estimated program effect on the sample over a larger group (assignees) or a smaller group (enrollees). Thus, the two sets of estimates are entirely consistent; they just measure different concepts. In the remainder of this Executive Summary, we focus on the estimated impacts per assignee, because they are the most reliable, direct experimental evidence of the effects of the program.

Impacts on earnings reflect program effects on both the amount of time treatment group members worked and how much they were paid per hour worked. Exhibit S.2 shows estimated impacts on the average number of hours worked by assignees and average earnings per hour worked over the follow-up period, expressed as percentages of the corresponding control group means. The percentage impacts on these two *components of earnings* approximately sum to the percentage impact on total earnings per assignee.[6]

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[4] To derive estimates for enrollees, it was necessary to assume that there was no impact on the earnings and employment of nonenrollees. There is evidence, however, that about half of all nonenrollees had some contact with the program after random assignment and received some—usually minimal—program services. As a result, the estimates in the bottom panel probably overstate somewhat the true impact on enrollees, while the estimated impacts per assignee understate the true impact on enrollees. Thus, the true impact on enrollees probably lies somewhere between these two estimates. The estimates for enrollees also adjust for the fact that 3 percent of the control group became enrolled in JTPA, despite the experiment's embargo on their participation

[5] As was true of the estimated impact per assignee for male youths, the large, negative impact per enrollee for male youths is due almost entirely to a very large estimated impact for those male youth enrollees with a previous arrest.

[6] Because the impacts on earnings per hour worked were estimated indirectly, we did not calculate significance levels for these impacts.

Exhibit S.2 Percentage Impacts on Total 18-Month Earnings and Its Components: JTPA Assignees and Enrollees, by Target Group

Percentage impact on:	*Adult women (1)*	*Adult men (2)*	*Female youths (3)*	*Male youths (4)*
Earnings per assignee	7.2%***	4.5%	-2.9%	-7.9%**
Hours worked per assignee	3.7	4.5**	-4.7	-6.8**
Earnings per hour worked	3.4[a]	0.0[a]	1.9[a]	-1.2[a]
Sample size	*6,474*	*4,419*	*2,300*	*1,748*

a. Tests of statistical significance were not performed for impacts on earnings per hour worked.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

As shown in Exhibit S.2, the 7.2 percent increase in earnings for adult women reflects a combination of a 3.7 percent increase in hours worked and a 3.4 percent increase in average hourly earnings among those who worked. The earnings gain for adult men, on the other hand, was entirely attributable to a 4.5 percent rise in hours worked, with no increase in hourly earnings.

Among female youths a -4.7 percent *reduction* in hours worked more than offset a 1.9 percent *increase* in hourly earnings to produce the negligible impact on total earnings that we saw earlier. Among male youths the -7.9 percent loss in total earnings was primarily attributable to a decrease in hours worked (of -6.8 percent).

Overall, then, JTPA appears to have had modest positive effects on the earnings and employment of adult women and men. But the program appears to have had virtually no effect on the earnings and employment of female youths and most male youths. In contrast, it may have had a large, negative impact on the earnings of those male youths who had been arrested before they applied to JTPA.

When estimated separately by site, positive but generally insignificant earnings effects were obtained in most sites for adult women and adult men, negative but generally insignificant earnings effects were obtained for male youths, and a majority of sites yielded negative but insignificant earnings effects for female youths (not shown here). Thus, the main 18-month earnings findings by target group were found to be widespread across the 16 SDAs in the study. And despite wide variation in the magnitude of these estimated effects, the sites did not differ significantly from one another in the degree to which JTPA affected earnings in any individual target group.

In an attempt to explain the variation in impact estimates across sites, we conducted a limited exploratory analysis of local factors that might influence program impacts. Three types of factors were considered: (1) characteristics of the JTPA programs; (2) prevailing labor market conditions; and (3) the types of persons accepted into the programs. But no clear patterns emerged from the analysis; and almost all of the findings were statistically insignificant, which is probably due to the small samples at each site and the limited number of sites involved.

Findings for Subgroups of Adult Women and Men

The impacts presented in the previous section are estimates of the average effects of the program on each target group in the study sample. Because JTPA provides a number of different employment and training services to a wide range of program applicants, it is important to analyze how program impacts varied with the types of services offered and the characteristics of the applicants. In this section we therefore present estimates of program impacts on the earnings of adult subgroups defined by the services that program intake staff recommended for them and by selected personal characteristics.

SERVICE STRATEGIES RECOMMENDED

For purposes of this analysis, members of the study sample were classified into three *service strategy subgroups* based on the services that program intake staff recommended for each sample member prior to random assignment.[7] Applicants recommended for classroom training in occupational skills were placed in the *classroom training* subgroup. Those recommended for on-the-job training (OJT) were placed in the *OJT/JSA* subgroup (so named because many of the treatment group members in this subgroup were enrolled in job search assistance while searching for either an on-the-job training position or an unsubsidized job). Because JTPA staff sometimes recommend combinations and sequences of services, applicants placed in either of these subgroups may also have been recommended for any of several other services, including job search assistance, basic education, work experience, or miscellaneous other services. Those applicants recommended for one or more of these services—but neither classroom training in occupational skills nor on-the-job training—were placed in the third subgroup: *other services*.[8]

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[7] Service strategy subgroups were defined based on the services recommended rather than the services received for two reasons. First, it was not possible to identify control group members who were comparable to the treatment group members who received particular JTPA services, whereas it *was* possible to identify control group members who were recommended for the same services as treatment group members. Second, and more fundamentally, since program staff can recommend services but cannot ensure that applicants participate in those services, recommended services represent the operative program decision to be evaluated.

[8] A few applicants designated for this other service subgroup were recommended for classroom training in occupational skills or on-the-job training as part of "customized training."

Exhibit S.3 Service Strategies Recommended: Adult JTPA Assignees, by Gender

Service strategy	*Adult women (1)*	*Adult men (2)*
Classroom training	44.0%	24.6%
OJT/JSA	35.0	48.7
Other services	21.0	26.7
Sample size	*4,465*	*3,759*

As shown in Exhibit S.3, nearly half of all adult men in the treatment group were recommended for the OJT/JSA service strategy, with the remainder about equally divided between the classroom training and other services strategies. Women were more likely than men to be recommended for classroom training (44 percent versus 25 percent) and less likely to be recommended for OJT/JSA (35 percent versus 49 percent).

It is important to note that program intake staff recommended services based on the individual applicants' employment needs and qualifications, as well as their personal preferences. The service strategy subgroups therefore differed from one another not only in terms of the service recommendations but also in terms of personal characteristics.

ENROLLMENT RATES AND DURATION, BY SERVICE STRATEGY SUBGROUP

After assessment and recommendation of services, two-thirds of the applicants accepted by intake staff were randomly assigned to the treatment group, which was allowed access to JTPA, and one-third were assigned to the control group, which was excluded from JTPA for 18 months.[9]

As noted above, not all treatment group members would ultimately become enrolled in JTPA. Enrollment rates differed by service strategy subgroup, but overall they were quite similar for adult women and men. Within the treatment group as a whole, 65 percent of adult women and 61 percent of adult men were enrolled in JTPA at some time during the 18-month follow-up period. Enrollment rates were highest in the classroom training subgroup (73 percent and 71 percent for adult women and men, respectively) and lowest in the OJT/JSA subgroup (55 percent and 57 percent).

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[9] This embargo on services to control group members was successfully implemented. Over the course of the 18-month follow-up period, only 3 percent of control group members became enrolled in JTPA.

The duration of enrollment in the program also differed by service strategy, ranging from a median length of about 2 months for women and men in the OJT/JSA and other services subgroups to median lengths of enrollment of 4 to 6 months in the classroom training subgroup. Generally, there was little difference by gender in the duration of enrollment except that women in classroom training tended to stay in the program about two months longer than men.

SERVICES RECEIVED, BY SERVICE STRATEGY SUBGROUP

Within the *classroom training subgroup* the most common JTPA services received by treatment group members who became enrolled in the program were classroom training in occupational skills, basic education, and job search assistance. Enrollees in the *OJT/JSA subgroup* were most likely to receive on-the-job training or job search assistance, or both. In the *other services subgroup* the most common services adults received were job search assistance and miscellaneous services, such as job-readiness training. Exhibit S.4 shows that between 82 percent and 89 percent of the enrollees in each service strategy subgroup received one or both of the two key services characteristic of that service strategy. Thus, the three service strategy definitions represent distinctly different mixes of services actually received, as well as services recommended.

The impacts of the program do not depend solely, however, on the JTPA services received by those in the treatment group. Instead, the impacts reflect the *difference* between the services received by those given access to JTPA and the services they would have received if they had been excluded from the program. That is, the benchmark against which we measure the effects of JTPA is the services available elsewhere in the community, not a total absence of services. Our measure of the services the treatment group would have received if they had been excluded from the program is those received by the control group, who were excluded from the program.

Since we measure impacts per assignee (treatment group member), the relevant comparison is in terms of services per assignee, including those who were never enrolled in JTPA. As expected, the largest treatment-control group difference in the *classroom training subgroup* was in receipt of classroom training in occupational skills. Among adult women 49 percent of the treatment group received this service, whereas only 29 percent of the control group did. Among adult men these figures were 40 percent versus 24 percent.

Adult treatment group members in the *OJT/JSA subgroup* were much more likely than control group members to receive on-the-job training. We estimate that 29 percent of the women and 27 percent of the men in the treatment group in this subgroup received OJT,

Exhibit S.4 *Key JTPA Services Received by Treatment Group Members Who Were Enrolled in the Program: Adults, by Gender and Service Strategy Subgroup*

	% of enrollees receiving one or both services	
Key services in service strategy subgroup	*Adult women (1)*	*Adult men (2)*
	Classroom training subgroup	
Classroom training in occupational skills/basic education[a]	88.8%	85.5%
	OJT/JSA subgroup	
On-the-job training/ job search assistance	87.8%	86.5%
	Other services subgroup	
Job search assistance/ miscellaneous[b]	82.3%	88.7%
Sample size	*2,883*	*2,286*

[a] "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

[b] "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

whereas less than 1 percent of the corresponding control groups received this service, since OJT is typically not funded by non-JTPA providers. We were not able to measure control group receipt of job search assistance from non-JTPA providers, and so we could not estimate the treatment-control group difference for that service.

As noted earlier, the most common JTPA services provided to adults in the *other services subgroup* were job search assistance and miscellaneous services. Around 25 percent of adult treatment group members in this subgroup received the former service, and about 30 percent received the latter. We were unable to measure receipt of these services from non-JTPA providers and therefore cannot estimate the treatment-control group difference.

JTPA thus represented a clear increment in the services available elsewhere in the community, at least in the classroom training and OJT/JSA subgroups where we could measure the treatment-control group differential, but that increment was relatively modest.

Among adult women and men the average assignee in the classroom training subgroup received only an additional 95 to 110 hours of classroom training in occupational skills, and the average assignee in the OJT/JSA subgroup likewise received only an additional 104 to 114 hours of on-the-job training.

IMPACTS ON EDUCATIONAL ATTAINMENT, BY SERVICE STRATEGY SUBGROUP

An intermediate effect of the increment in services received by treatment group members was an increase in educational attainment among those who were high school dropouts. Dropouts made up around 30 percent of the adult target groups. Our analysis focuses on the *attainment of a training-related high school credential*, which we define as both having received a school or training service *and* having received a high school diploma or General Educational Development (GED) certificate at some time during the 18-month follow-up period.

As might be expected, the increase in educational attainment was greatest among those dropouts recommended for the *classroom training* service strategy. Exhibit S.5 indicates that nearly 30 percent of the adult dropouts in the classroom training treatment group received a training-related high school credential, whereas only 11 percent of the control group did—for impacts that were highly significant in the cases of both genders. There were smaller, but still statistically significant, increases in the proportions of female dropouts in the *other services subgroup* and male dropouts in the *OJT/JSA subgroup* who attained a high school credential as a result of the program. But there were no significant effects on educational attainment among women in the OJT/JSA subgroup or men in the other services subgroup.

Exhibit S.5 Impacts on Attainment of a Training-Related High School Diploma or GED Certificate: Adult JTPA Assignees Who Were High School Dropouts, by Gender

	Adult women			*Adult men*		
	% attaining HS/GED		*Impact, in*	*% attaining HS/GED*		*Impact, in*
Service strategy subgroup	*Assignees (1)*	*Controls (2)*	*% points (3)*	*Assignees (4)*	*Controls (5)*	*% points (6)*
Classroom training	29.2%	11.3%	17.9***	27.3%	11.3%	16.0***
OJT/JSA	9.1	10.9	-1.8	8.4	4.4	4.0**
Other services	17.4	9.8	7.6**	10.2	8.7	1.5
All subgroups	19.1	10.8	8.2***	12.7	6.7	6.0***
Sample size[a]			*1,515*			*1,258*

[a] Assignees and control group members who were high school dropouts.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

IMPACTS ON EARNINGS, BY SERVICE STRATEGY SUBGROUP

Exhibit S.6 shows the estimated program impacts on the earnings of adult women and men in each service strategy subgroup. As shown in the second column of the top panel of the exhibit, impacts on the earnings of adult women in the *classroom training subgroup* followed the expected pattern for this type of service: an earnings loss in the first quarter, representing an initial investment of time in training, followed by a payback period of rising earnings gains in the next five quarters, with statistically significant gains of $144 and $188 in the last two quarters of the follow-up period. The overall 18-month earnings gain of $398 for women in this *subgroup* was not statistically significant. This gain reflected an estimated 8.9 percent program-induced increase in the hourly earnings of those women who worked, which more than offset an insignificant -2.5 percent drop in the average number of total hours worked by all adult women over the follow-up period (estimates not shown in the exhibit).

The estimated impacts on the earnings of adult men in the classroom training subgroup are less clear. None of the impacts on quarterly earnings was significantly different from zero, nor was the overall impact on total earnings over the follow-up period. Moreover, the program had no significant impact on the employment rate or hours of work over the follow-up period for this subgroup of men (estimates not shown). Thus, there is no evidence of a program impact on the earnings and employment of this subgroup.

In contrast to the pattern for women in the classroom training subgroup, women in the *OJT/JSA subgroup* (middle panel of the exhibit) experienced an immediate and sustained positive impact on average earnings throughout the follow-up period, as might be expected with a strategy that emphasizes immediate placement in either an on-the-job training position or a regular job. Women in the OJT/JSA subgroup had significant quarterly earnings impacts of $109 to $144 in five of the six quarters, with a significant gain of $742 over the follow-up period as a whole.

Men in the OJT/JSA subgroup experienced estimated gains of similar magnitude in five of the six quarters and over the follow-up period as a whole, although the estimated impacts were less often statistically significant. Over the 18 months men in this subgroup experienced significant earnings gains of $781.

Both women and men in the OJT/JSA subgroup experienced a positive and significant impact on hours worked; and men, on their employment rate (estimates not shown in the exhibit). Indeed, the earnings gains of both women and men in this subgroup were due primarily to increases in the number of hours worked per sample member, rather than to higher hourly earnings while employed.

Exhibit S.6 *Impacts on Quarterly and 18-Month Earnings: Adult JTPA Assignees, by Gender and Service Strategy Subgroup*

	Adult women		*Adult men*	
Period	*Control mean (1)*	*Impact per assignee (2)*	*Control mean (3)*	*Impact per assignee (4)*
		Classroom training subgroup		
Quarter 1	$ 714	$ -70*	$ 1,440	$ -101
2	938	5	1,714	126
3	1,066	52	1,884	213
4	1,189	79	2,184	50
5	1,253	144**	2,171	151
6	1,230	188***	2,387	- 21
All quarters	6,391	398	11,780	418
Sample size [a]		*2,847*		*1,057*
		OJT/JSA subgroup		
Quarter 1	$ 1,143	$ 144***	$ 1,757	$ 54
2	1,379	81	2,014	135
3	1,449	129**	2,133	164*
4	1,520	109*	2,199	94
5	1,546	142**	2,183	133
6	1,570	138**	2,169	201**
All quarters	8,607	742**	12,456	781*
Sample size [a]		*2,287*		*2,250*
		Other services subgroup		
Quarter 1	$ 960	$ 39	$ 1,677	$ 74
2	1,198	132	1,951	104
3	1,248	220**	2,123	44
4	1,471	22	2,199	44
5	1,535	2	2,292	13
6	1,548	42	2,274	- 19
All quarters	7,960	457	12,516	261
Sample size [a]		*1,340*		*1,112*

[a] Assignees and control group members combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

In contrast to the sustained, positive impact on earnings in the OJT/JSA subgroup, the program appears to have had only a short-lived effect on the earnings of adult women, and virtually no effect on the earnings of adult men, in the *other services subgroup* (bottom panel). JTPA had a significant impact on women's earnings of $220 in the third quarter, followed by much smaller, insignificant gains in the later quarters. The estimated impacts on hours worked quarterly (not shown) mirrored this pattern—possibly reflecting quicker placement in jobs that were similar to those the female assignees would have eventually found without access to JTPA. For men in the other services subgroup, neither the estimated impacts on quarterly earnings nor the estimated impacts on hours of work (not shown) were statistically significant.

Overall, then, JTPA led to modest, statistically significant earnings gains in at least one quarter for adult women in all three service strategies. The timing of impacts was very different across the subgroups, however, and significant for the follow-up period as a whole only in the OJT/JSA subgroup. Significant impacts on the earnings of adult men were concentrated exclusively in the OJT/JSA subgroup.

It is important to iterate that the adults in the three service strategy subgroups differed not only in the services they received, but also in their personal characteristics. Program intake staff tended to recommend the most employable applicants for the OJT/JSA service strategy. This difference is evident not only in the data on baseline characteristics of the three subgroups (not shown here) but also in the earnings of control group members over the follow-up period, shown in columns (1) and (3) of Exhibit S.6. These figures indicate that in the absence of program services women recommended for OJT/JSA would have earned substantially more than those recommended for classroom training and somewhat more than those recommended for other services. Among men the more job-ready applicants tended to be recommended for either OJT/JSA or other services; those male control group members recommended for classroom training earned somewhat less over the follow-up period than either of the other two subgroups.

Because of these differences in the three subgroups, one cannot extrapolate the impacts for one service strategy subgroup to the women or men served by another. We cannot, for example, conclude that the program outcomes for adult men in the classroom training subgroup would have been better if instead they had been recommended for the OJT/JSA service strategy. We can only determine which service strategies were effective *for those applicants recommended for them*. Whether another service strategy would have been more effective cannot be determined on the basis of this study, since we did not observe alternative service approaches applied to comparable participant populations.

It is also important to bear in mind that the costs, as well as the impacts, of the three service strategies were likely to have varied, as may the longer term impacts. In our final report on this study we will present an analysis that compares the costs of Title II-A to its impacts over a longer follow-up period.

IMPACTS ON EARNINGS, BY ETHNICITY AND BARRIERS TO EMPLOYMENT

In addition to the three subgroups defined based on service strategy recommendations, we estimated program impacts on the 18-month earnings of a number of other subgroups of women and men, defined in terms of personal characteristics measured upon their application to the program. These estimates helped us determine whether the impacts of the program were concentrated within certain groups of interest to policymakers and program planners or broadly distributed across all adult women or men. In this Executive Summary we present the results for two such key subgroups: the major *ethnic groups*, and groups facing different *barriers to employment.*[10]

Exhibit S.7 presents the estimated program impacts on the earnings of *white, black,* and *Hispanic* women (column 3) and men (column 6). Among women the estimated impacts appear to have differed noticeably by ethnic group, with white women showing significant earnings gains of $723 over the 18-month follow-up period; black women, an insignificant earnings gain of $457; and Hispanic women, an insignificant loss of $-414. Moreover, separate tests of the statistical significance of the differences *among* these impact estimates (not shown) indicate the differences were statistically significant at near-conventional levels. The estimated impacts for adult men also differed by ethnic group, but neither the estimated impacts for individual ethnic groups nor the differences in impacts among the subgroups were statistically significant and therefore could have arisen by chance.

In an attempt to narrow the range of possible explanations for the differences in estimated impacts for women in different ethnic groups, we estimated adjusted impacts that controlled for differences in the distributions of these subgroups across study sites and across service strategy subgroups. When we controlled for differences in the distributions of the three ethnic groups of women across the *study sites,* the estimated impacts for these groups were not significantly different from one another. This finding suggests that the differences in estimated impacts among women in different ethnic groups are in part attributable to *differences in the distributions* of these groups across sites. In addition, given the extreme concentration of Hispanic women in a few sites we cannot reliably distinguish negative effects on Hispanic women as an ethnic group from negative effects on *all* women in one or more of the sites in which Hispanic women were concentrated.

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[10] Other key subgroups examined in the report include those defined by work and training histories, public assistance histories, household income and composition, public housing status, and age.

Exhibit S.7 *Impacts on the 18-Month Earnings of Major Ethnic Groups: Adult JTPA Assignees, by Gender*

	Adult women			*Adult men*		
Ethnic group	*Sample size*[a] *(1)*	*Control mean* *(2)*	*Impact per assignee* *(3)*	*Sample size*[a] *(4)*	*Control mean* *(5)*	*Impact per assignee* *(6)*
White, non-Hispanic	3,541	$ 8,007	$ 723***	2,668	$ 12,929	$ 625
Black, non-Hispanic	1,981	6,829	457	1,155	10,931	957
Hispanic	744	6,775	-414	400	13,555	-741
Full sample [b]	6,474	7,488	539***	4,419	12,306	550

a. Assignees and control group members combined.
b. Including the three major ethnic groups and American Indians, Alaskan Natives, Asians, and Pacific Islanders.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

To determine whether the effects of the program varied with the degree of labor market disadvantage, we also estimated impacts on earnings for subgroups defined by three barriers to employment: *welfare receipt, limited education,* and *limited recent work experience.*[11] The first three pairs of rows of Exhibit S.8 show the estimated impacts on earnings for women and men facing each of these barriers and for those who were not.

The mean 18-month earnings of control group members, shown in columns 2 and 5 of the exhibit, illustrate that these barriers were indeed serious obstacles to employment. Control group members in all three subgroups facing these barriers earned much less over the follow-up period than those who were not.

Among both women and men the estimated impacts tended to be larger for those *not* facing the barriers in question, although among women the differences in impacts between those facing and those not facing a particular barrier were smaller than the differences among men. Separate tests for the significance of these differences between each *pair* of estimates indicated, however, that any differences shown here may have arisen by chance.

Because some persons who were facing one of these barriers to employment may also have been facing one or both of the other barriers, these subgroups overlap to some degree. To achieve a clearer distinction among the subgroups in terms of the overall difficulty of becoming employed, the bottom panel of Exhibit S.8 categorizes the women

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[11] *Welfare receipt* is defined as receiving Aid to Families with Dependent Children (AFDC), General Assistance, or any other cash welfare benefits upon application to JTPA. *Limited education* is defined as lack of a high school diploma or GED certificate; *limited recent work experience* is defined as having worked less than 13 weeks in the year prior to application to JTPA. These three measures of barriers to employment are similar to those used in other recent studies of JTPA programs (see U.S. General Accounting Office, 1989).

Exhibit S.8 *Impacts on the 18-Month Earnings of Subgroups Facing Selected Barriers to Employment: Adult JTPA Assignees, by Gender*

	Adult women			*Adult men*		
Barrier to employment (in italic)	*Sample size*[a] *(1)*	*Control mean (2)*	*Impact per assignee (3)*	*Sample size*[a] *(4)*	*Control mean (5)*	*Impact per assignee (6)*
Receiving cash welfare	2,446	$ 5,492	$ 387	611	$ 9,541	$ -46
No cash welfare	3,500	8,965	697***	3,788	13,032	624*
No high school diploma or GED certificate	1,731	6,072	416	1,249	10,353	398
High school diploma or GED certificate	4,316	8,064	681***	2,873	13,335	878**
Worked less than 13 weeks in past 12 mos.	3,022	5,555	511**	1,614	10,478	-210
Worked 13 weeks or more in past 12 mos.	2,622	9,956	668**	2,392	14,320	787*
Number of barriers						
None of the above	1,361	10,971	909**	1,465	15,142	1,203**
One of the above	1,655	7,950	802**	1,550	12,184	194
Two of the above	1,435	5,756	379	617	9,044	30
All three of the above	488	3,703	-213	116	8,595	-146
Full sample	6,474	7,488	539***	4,419	12,306	550

a Assignees and control group members combined.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

and men in the sample by the *number* of these barriers they were facing. Again, the average earnings of the control groups indicate that this categorization is strongly predictive of what JTPA assignees would have earned without the program: control group earnings fall steadily as the number of barriers rises.

For both women and men the impacts were the largest in the subgroup facing none of the three barriers. For neither women nor men, however, were the differences in impacts *among* subgroups statistically significant; thus, these differences may merely reflect sampling error.[12]

SUMMARY AND COMPARISON WITH PREVIOUS FINDINGS

Overall, JTPA Title II-A had a modest positive impact on the earnings of adult women over the follow-up period: on average, a significant gain of $539 over the 18 months following their application. The estimated earnings gain for men was similar ($550) but was not statistically significant. These overall averages mask substantial variation in both the magnitude and time patterns of program impacts among subgroups of women and men, however.

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[12] Among the adult female subgroups, for example, there is a 46 percent chance of finding differences at least as large as those shown here even if there were no true differences in impacts among the subgroups.

When adult women are categorized by the service strategy recommended by program intake staff, the only ones to experience a statistically significant earnings impact over the follow-up period as a whole were those in the OJT/JSA subgroup, with a gain of $742. Women in this subgroup enjoyed consistently positive, statistically significant earnings increases of $109 to $144 in five of the six follow-up quarters. Women in the classroom training subgroup experienced an earnings loss in the first calendar quarter of the follow-up period, followed by growing positive impacts, and culminating in significant impacts of $144 and $188 in the fifth and sixth quarters. Program impacts on the earnings of women in the other services subgroup were significant only in the third quarter, when these women gained $220, on average; impacts for this subgroup were negligible in subsequent quarters.

Impacts for adult men were similar in magnitude to those for women, although they were less frequently statistically significant. As with the women, only those in the OJT/JSA subgroup enjoyed significant earnings gains (of $781) over the follow-up period as a whole. Estimated impacts on the earnings of men in the classroom training and other services subgroups were never statistically significant, either for the follow-up period as a whole or for individual quarters.

These impact estimates are similar in magnitude to those found in the few previous evaluations that have used rigorous experimental designs. For example, studies of state work-welfare programs for women in the early 1980s found significant positive impacts in the first two years after random assignment that ranged up to about $250 per quarter.[13] Evaluations of demonstration programs for displaced workers in Texas and New Jersey found similar impacts on the earnings of men—that is, in the same range but not statistically significant—in the first year after random assignment.[14]

Comparisons with the results of earlier studies are complicated, however, by the fact that the programs involved in those studies provided somewhat different services from those in JTPA and served primarily subpopulations such as welfare recipients and (for men) displaced workers and ex-addicts. Moreover, the programs for women examined in earlier studies were, unlike JTPA, mostly mandatory, and yet had lower rates of participation in employment and training services than those of our study sample.

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[13] See Gueron and Pauly (1991).

[14] See Bloom (1990) and Corson et al. (1989).

Finally, when adult women in the National JTPA Study sample were classified by ethnic group, differences in estimated impacts on earnings were emerged, with white women experiencing greater gains than minority women, particularly Hispanic women. Further tests revealed, however, that these differences in impacts may well have been due to the concentration of Hispanic women in a few sites that experienced impacts substantially below the average for all women. There were no significant differences in impacts on the earnings of adult men by ethnic group. Impact estimates for adults who were and were not subject to various barriers to employment were not statistically significantly different from each other. But the pattern of estimates for these subgroups suggested that JTPA produced larger positive impacts for adults with fewer labor market barriers.

Findings for Subgroups of Female and Male Out-of-School Youths

Out-of-school youths in the study sample were classified into the same three service strategy subgroups as those used to classify adults: classroom training, OJT/JSA, and other services. These subgroups were based on the JTPA services recommended for sample members by program intake staff before random assignment.

SERVICE STRATEGIES RECOMMENDED

The service strategies recommended for youths reflect a difference in emphasis between JTPA Title II-A programs for youths and those for adults. Programs for adults emphasize employment, as evidenced by the fact that program performance standards for adults are based largely on job placement rates. In contrast, programs for youths emphasize a broader range of outcomes, with performance standards for youths based in part on "positive terminations," which include not only job placements but also participation in further training and attainment of specific job competencies.

A comparison of Exhibit S.9 and the earlier Exhibit S.3 indicates that youths were far less likely than adults to be recommended for the *OJT/JSA strategy*, especially if we compare female youths with female adults and male youths with male adults. Of the three service strategies OJT/JSA places the greatest emphasis on immediate employment; thus, this difference between youths and adults reflects the difference between JTPA programs for the two age groups. In addition, youths were much more apt than adults to be recommended for the *other services strategy*, which, as discussed below, also differed between the two age groups in the mix of program services *received*.

Service strategy recommendations also differed between female and male youths themselves. Female youths were more likely than male youths to be recommended for

Exhibit S.9 Service Strategies Recommended: Out-of-School Youth JTPA Assignees, by Gender

Service strategy	*Female youths (1)*	*Male youths (2)*
Classroom training	44.3 %	29.9 %
OJT/JSA	23.2	32.9
Other services	32.5	37.3
Sample size	*1,814*	*1,436*

classroom training (44 percent versus 30 percent, respectively) and less likely than male youths to be recommended for OJT/JSA (23 percent versus 33 percent). The genders were about equally likely to be recommended for other services (33 percent versus 37 percent).

ENROLLMENT RATES AND DURATION, BY SERVICE STRATEGY SUBGROUP

Enrollment rates overall were comparable to those for adults, with 65 percent of the female youth treatment group and 67 percent of the male youth treatment group becoming enrolled in JTPA Title II-A at some time during the 18-month follow-up period. Treatment group enrollment rates were highest in the classroom training subgroup (71 percent for females and 75 percent for males). The lowest enrollment rates were in the OJT/JSA subgroup (57 percent for females and 58 percent for males). The other services subgroup fell between these two extremes, with enrollment rates of 63 percent for female and 68 percent for male youth treatment group members.

Out-of-school youths who enrolled in JTPA stayed in the program slightly longer than their adult counterparts, with the median duration of enrollment at 3.9 months for female youths (versus 3.6 months for adult women) and at 3.1 months for male youths (versus 2.5 months for adult men). Thus, the median duration of enrollment was also slightly longer for female than for male youths. The service strategy subgroup with the shortest enrollments was OJT/JSA, with a median of about 2 months for both females and males; the classroom training subgroup had the longest enrollments, at 5.5 months for females and 4.6 months for males. The median for the other services subgroup was about 3 months for both target groups.

Exhibit S.10 *Key JTPA Services Received by Treatment Group Members Who Were Enrolled in the Program: Out-of-School Youths, by Gender and Service Strategy Subgroup*

	% of enrollees receiving one or both services	
Key services in service strategy subgroup	*Female youths (1)*	*Male youths (2)*
	Classroom training subgroup	
Classroom training in occupational skills/basic education [a]	86.1%	80.4%
	OJT/JSA subgroup	
On-the-job training/ job search assistance	84.8%	84.5%
	Other services subgroup	
Basic education/ [a] miscellaneous [b]	79.5%	83.2%
Sample size	*1,188*	*959*

[a] "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

[b] "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

SERVICES RECEIVED, BY SERVICE STRATEGY SUBGROUP

Exhibit S.10 shows the percentage of enrollees in each service strategy subgroup who received one or both of the key services in that service strategy. About 86 percent of female youth enrollees and about 80 percent of male youth enrollees recommended for classroom training received classroom training in occupational skills, basic education, or both. About 85 percent of the female and male youth enrollees in the OJT/JSA subgroup received on-the-job training, job search assistance, or both. And about 80 percent of the female and 83 percent of the male youth enrollees in the other services subgroup received basic education, miscellaneous services, or both.

The only obvious difference between the mix of JTPA services received by youths and the mix received by adults was in the other services subgroup. Whereas adult enrollees in this subgroup received mainly job search assistance and miscellaneous services (Exhibit S.4), the youth enrollees received mainly basic education and miscellaneous services—further evidence, as noted earlier, that JTPA emphasizes immediate employment for adults more than it does for youths.

As with the adults in our sample, the estimated program *impacts* on youths reported below reflect *differences* in the employment and training services received by treatment group members, who had access to JTPA, and the services they would have received if they had been excluded from the program, as measured by data on control group members. And as with adults, the size of these treatment-control group differences in service receipt varied by service strategy subgroup.

In the *classroom training subgroup* about 48 percent of the female youths and 43 percent of the male youths in the treatment group received classroom training in occupational skills, whereas only 31 percent of the female youths and 22 percent of the male youths in the control group received this service. In the *OJT/JSA subgroup* about 30 percent of the females and 31 percent of the males in the treatment group received on-the-job training, while less than 1 percent of both females and males in the control group received the service.

We were unable to measure the control group's receipt of miscellaneous services—the most common category of services received by youth treatment group members in the *other services subgroup,* at 29 percent for females and 35 percent for males. It is therefore not possible to determine the treatment-control group difference in service receipt for this key service in the subgroup. The service differential was small, however, for basic education, the other key service received by youth treatment group members who were recommended for the other services strategy. About 23 percent of female youths and 14 percent of male youths in the treatment group received basic education, while 19 percent of the females and 12 percent of the males in the control group received it.

Thus, JTPA produced a noticeable increment in service receipt in the two service strategy subgroups for which we could measure the differences: classroom training and OJT/JSA. In the third, other services, we could not measure the treatment-control group differential for the most common service received by the treatment group and found only a slight differential in the case of the other key service. In terms of the average number of hours of services received, JTPA produced a modest increase for the two subgroups for which we could measure this effect. Female and male youths in the classroom training subgroup received, respectively, 187 and 127 more hours of classroom training in occupational skills than they would have if JTPA were not available, while female and male youths in the OJT/JSA subgroup received, respectively, an additional 105 and 128 hours of on-the-job training.

IMPACTS ON EDUCATIONAL ATTAINMENT, BY SERVICE STRATEGY SUBGROUP

Differences in the services received by youths in the treatment and control groups produced differences in the rate at which high school dropouts in these groups attained a high school

Exhibit S.11 *Impacts on Attainment of a Training-Related High School Diploma or GED Certificate: Out-of-School Youth JTPA Assignees Who Were High School Dropouts, by Gender*

Service strategy subgroup	Female youths: % attaining HS/GED Assignees (1)	Female youths: % attaining HS/GED Controls (2)	Female youths: Impact, in % points (3)	Male youths: % attaining HS/GED Assignees (4)	Male youths: % attaining HS/GED Controls (5)	Male youths: Impact, in % points (6)
Classroom training	32.9%	16.6%	16.4***	27.3%	18.3%	9.0*
OJT/JSA	9.8	6.0	3.8	14.9	4.9	10.1***
Other services	31.7	21.0	10.7**	26.1	16.9	9.1**
All subgroups	28.6	16.6	11.9***	23.9	14.0	9.9***
Sample size [a]			*1,050*			*955*

[a] Assignees and control group members who were high school dropouts.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

diploma or GED certificate. Since half of the female youths in the study sample and three-fifths of the male youths were high school dropouts, impacts on their educational attainment represent an important result of the program.

As shown in the fourth row of Exhibit S.11, among control group members who were dropouts 17 percent of the female youths and 14 percent of the male youths both enrolled in a school or training service and received a high school diploma or GED certificate at some time during the 18-month follow-up period. Among the corresponding treatment group members, however, 29 percent of the female youths and 24 percent of the male youths subsequently attained a training-related high school credential. The program impact in both cases was highly significant. Impacts were also statistically significant for male youths in all three service strategy subgroups and for females in the classroom training and other services subgroups—the two service strategy subgroups that focused the most on basic education. The impact was particularly striking for female youths in the classroom training subgroup.

IMPACTS ON EARNINGS, BY SERVICE STRATEGY SUBGROUP

As noted at the outset, the estimated program impact on the earnings of female youths overall was negligible; the impact on male youths overall was substantially negative, but that impact was largely concentrated among those male youths who reported having been arrested between their sixteenth birthday and random assignment. Exhibit S.12 provides a more detailed understanding of these findings by presenting estimates for the three service strategy subgroups of youths during each of the six quarters of the follow-up period.

Exhibit S.12 *Impacts on Quarterly and 18-Month Earnings: Out-of-School Youth JTPA Assignees, by Gender and Service Strategy Subgroup*

	Female youths		*Male youths*	
Period	*Control mean (1)*	*Impact per assignee (2)*	*Control mean (3)*	*Impact per assignee (4)*
		Classroom training subgroup		
Quarter 1	$ 742	$ -210***	$ 1,226	$ -300**
2	909	- 189***	1,345	96
3	1,052	-150*	1,655	- 2
4	991	24	1,773	0
5	1,047	70	1,889	- 56
6	1,196	- 87	1,895	4
All quarters	5,936	- 542	9,783	- 259
Sample size [a]		*1,045*		*526*
		OJT/JSA subgroup		
Quarter 1	$ 1,002	$ 149	$ 1,651	$ -57
2	1,074	203*	1,988	- 219
3	1,252	97	2,197	- 302*
4	1,363	3	2,160	- 203
5	1,368	103	2,316	- 192
6	1,562	-146	2,452	- 339**
All quarters	7,620	410	12,765	- 1,313*
Sample size [a]		*545*		*615*
		Other services subgroup		
Quarter 1	$ 653	$ 43	$ 1,362	$ -285**
2	909	-68	1,457	- 121
3	1,023	-96	1,605	- 218
4	1,047	-52	1,751	- 276*
5	1,093	-41	1,766	- 114
6	1,001	55	1,899	- 292**
All quarters	5,726	-158	9,839	- 1,305*
Sample size [a]		*710*		*607*

[a] Assignees and control group members combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test)

In the female *classroom training subgroup* impacts on earnings were negative and statistically significant during the first three follow-up quarters. As with adult women, these initial losses probably reflect the earnings forgone by treatment group members while they were attending classes. Unlike the experience of adult women, however, female youths in classroom training did not experience any significant increases in earnings later in the follow-up period. Hence, the earnings female youths lost while participating in classroom training were not offset by a payback period, at least not by the end of the 18-month follow-up.

Female youths in the *OJT/JSA subgroup* experienced a different pattern. The initial impacts on their earnings were moderately positive (and statistically significant in the second follow-up quarter), which may reflect an initial boost in employment produced by on-the-job training, job search assistance, or both. But these short-run gains were not sustained over time.

Program impacts on the earnings of female youths in the *other services subgroup* were negligible in all six follow-up quarters. In other words, the mix of predominantly miscellaneous services and basic education that JTPA provided to this subgroup had little or no impact.

The impact estimates for male youths in the *classroom training subgroup* were similar to those for their female youth counterparts. Impacts were substantially negative and statistically significant in the first follow-up quarter, again, perhaps reflecting the costs of being in class instead of employed. And as with female youths, the later follow-up quarters brought no earning increases large enough to offset the initial loss.

Impacts on male youths in the *OJT/JSA subgroup* were negative in all six follow-up quarters. Over the follow-up period as a whole the OJT/JSA strategy yielded a statistically significant earnings loss of $-1,313, or -10.3 percent of the corresponding control group's mean earnings. This loss reflected mainly an estimated -8.5 percent program-induced reduction in the average number of hours worked by male youths; average hourly earnings among those who worked were largely unaffected by the program (not shown in the exhibit).

Male youths in the *other services subgroup* experienced an estimated earnings loss of $-1,305, or -13.3 percent of what their earnings would have been without access to JTPA. This loss reflected mainly a -9.7 percent reduction in the average number of hours worked, although average hourly earnings when working were also reduced by an estimated -4.0 percent (not shown).[15]

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[15] The percentage impacts on hours worked and on earnings per hour worked do not sum exactly to the percentage impact on total earnings because the relationship between total earnings and its components is multiplicative, not additive.

For these last two service strategy subgroups of male youths, it therefore appears that the negative program impact on earnings reflected mainly a negative program impact on the number of hours worked, as opposed to a negative impact on the hourly earnings of those who worked. The next subsection will also demonstrate that the negative impacts on earnings for the OJT/JSA and the other services subgroups of male youths are attributable primarily to a large negative estimated impact on the earnings of those male youths with a previous arrest who were recommended for each of these two service strategies.[16]

It is important to bear in mind that although this analysis by service strategy subgroup is illuminating, one cannot interpret the findings for one service strategy subgroup as having direct implications for the youths recommended for one of the other two service strategies. Again, we can only determine which service strategies were effective for those applicants recommended for them, because the three service strategy subgoups differed in the personal characteristics of their members.

IMPACTS ON EARNINGS, BY ETHNICITY, BARRIERS TO EMPLOYMENT, AND REPORTED ARRESTS

The estimated program impacts on earnings for out-of-school youths did not vary systematically with the *ethnic backgrounds* of sample members or with the *barriers to employment* they faced when they applied to JTPA.

Exhibit S.13 presents the estimated program impacts on *white, black,* and *Hispanic* youths. The impact estimates for female youths did not differ substantially by ethnic group, and no ethnic group experienced a statistically significant impact. In addition, separate tests of the statistical significance of the differences among the impacts on these groups (not shown) confirm the lack of a differential effect of JTPA. For the male youths there were differences in estimated impacts among the three ethnic groups, but these differences were not statistically significant and may therefore have been due to chance (test not shown).

Exhibit S.14 presents the estimates for subgroups of youths defined in terms of the three specific barriers to employment investigated for adults: *welfare receipt, limited education,* and *limited recent work experience.* As was the case for adults, these barriers represented serious obstacles to employment for youths, as evidenced by the fact that

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[16] Note that the much smaller and statistically insignificant estimated impact on the earnings of male youths in the classroom-training subgroup is not attributable to this subgroup's having a substantially smaller proportion of previous arrestees than the other two service strategy subgroups (which it did not).

Exhibit S.13 Impacts on the 18-Month Earnings of Major Ethnic Groups: Out-of-School Youth JTPA Assignees, by Gender

	Female youths			Male youths		
Ethnic group	Sample size[a] (1)	Control mean (2)	Impact per assignee (3)	Sample size[a] (4)	Control mean (5)	Impact per assignee (6)
White, non-Hispanic	1,148	$ 7,076	$ -122	946	$ 12,550	$ -1,333**
Black, non-Hispanic	749	5,601	-135	522	8,164	75
Hispanic	366	5,019	-554	248	10,126	-1,238
Full sample[b]	2,300	6,225	-182	1,748	10,736	- 854**

[a] Assignees and control group members combined.

[b] Including the three major ethnic groups and American Indians, Alaskan Natives, Asians, and Pacific Islanders.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

control group earnings drop markedly as the number of barriers increases (bottom panel of the exhibit).

There was no statistically significant relationship, however, between the number or nature of these employment barriers and the effect of JTPA on out-of-school youths. Among female youths, in particular, there was little difference between the estimated program impact on sample members who faced each of the three employment barriers and those who did not face that barrier. Furthermore, there was no clear pattern in the relationship between the estimated program impacts and the *number* of employment barriers faced. Tests for significant differences in impacts *among* subgroups (not shown) revealed none that was statistically significant.

Among male youths the differences between the impact on sample members who faced a particular employment barrier and those who did not appear to have been more substantial. For male youths with limited education or limited recent work experience, JTPA appears to have reduced the earnings of those facing one of these two barriers by more than it reduced the earnings of those who did not (top panel, column 6). In addition, the more barriers faced, the more JTPA seems to have reduced earnings over the follow-up period. None of these differences in impact estimates between or among the subgroups were statistically significant, however, and so the patterns they imply are only suggestive and may in fact be due to chance. Moreover, the difference in impacts was in the opposite direction for male youths receiving welfare and those not receiving welfare.

Again, the most striking subgroup difference for youths was between the impacts estimated for male youths who had been arrested before and those who had not. As shown in Exhibit S.15, on average, male youths with a previous arrest experienced a highly significant $-3,038 program-induced earnings loss during their 18-month follow-up period. In contrast, male youths without a previous arrest experienced an insignificant

Exhibit S.14 Impacts on the 18-Month Earnings of Subgroups Facing Selected Barriers to Employment: Out-of-School Youth JTPA Assignees, by Gender

	Female youths			Male youths		
Barrier to employment (in italic)	*Sample size[a] (1)*	*Control mean (2)*	*Impact per assignee (3)*	*Sample size[a] (4)*	*Control mean (5)*	*Impact per assignee (6)*
Receiving cash welfare	701	$ 4,397	$ -391	185	$ 8,815	$ -56
No cash welfare	1,412	7,174	-154	1,374	11,292	-1,020**
No high school diploma or GED certificate	1,047	4,192	23	947	10,087	-1,144*
High school diploma or GED certificate	1,146	8,055	-437	730	11,612	-420
Worked less than 13 weeks in past 12 mos.	1,235	4,425	-31	754	8,616	-1,286**
Worked 13 weeks or more in past 12 mos.	829	8,886	-255	842	12,808	-832
Number of barriers						
None of the above	545	9,964	-260	475	13,352	-459
One of the above	790	6,552	-236	733	10,810	-695
Two of the above	675	4,486	-451	455	8,520	-1,242
All three of the above	281	2,189	659	81	7,642	-1,278
Full sample	2,300	6,225	-182	1,748	10,736	-854**

[a] Assignees and control group members combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

$-224 earnings loss. The *difference* between these two impact estimates was highly significant and did not change when we controlled for the distributions of the two subgroups across the study sites and service strategy subgroups.

Moreover, this difference appeared in all six follow-up quarters, all three service strategy subgroups, and 13 of the 15 study sites where youths were included in the sample.[17] The negative impact on the subgroup of male youths with a previous arrest (25 percent of the male youth treatment group) accounts for 82 percent of the program-induced earnings loss for male youths overall.

It is important to note, however, that these large, negative impact estimates, which are based on our First Follow-up Survey (the basis for all the impact estimates in this report), differ substantially from corresponding impact estimates for male youths with a previous arrest that are based on earnings data from an alternative data source, namely, the

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[17] The Oakland site excluded youths from the study, yielding a total of 15 study sites for the youth analysis.

Exhibit S.15 Impacts on the 18-Month Earnings of Subgroups With and Without a Previous Arrest: Out-of-School Youth JTPA Assignees, by Gender

	Female youths			*Male youths*		
Arrest status	*Sample size*[a] *(1)*	*Control mean (2)*	*Impact per assignee (3)*	*Sample size*[a] *(4)*	*Control mean (5)*	*Impact per assignee (6)*
Arrested since age 16	125	$ 5,827	$ 705	401	$ 11,237	$ -3,038***
Not arrested since age 16	2,122	6,251	-200	1,313	10,696	-224

a. Assignees and control group members combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

administrative records of state unemployment insurance agencies.[18] Impact estimates based on UI earnings data for a subsample of the 18-month study sample suggest there was virtually no program impact on the earnings of the previous arrestees among male youths. Although there is thus some question about the degree to which JTPA *reduced* the earnings of those male youths with a previous arrest, both data sources agree that the program *did not increase* their earnings, or the earnings of male youths overall.

We will explore further the differences in the estimates from the two data sources in our forthcoming final report. The impact estimates from the two data sources do not, however, differ appreciably for adult women, adult men, female youths, or those male youths who did not report a previous arrest.

SUMMARY AND COMPARISON WITH PREVIOUS FINDINGS

The preceding analysis has shown that JTPA Title II-A did not appreciably affect the earnings of female out-of-school youths. On average, the program reduced total earnings during the 18-month follow-up period by $-182 per female youth assignee (treatment group member), but this estimated effect was not statistically significant. Nor were the impact estimates statistically significant for female youths in each of the three service strategy subgroups or in any of the subgroups defined by personal characteristics.

The findings for male out-of-school youths are very different. On average, JTPA reduced the estimated earnings of this target group by a statistically significant $-854 over the 18-month follow-up period. But most of this negative estimated impact was concentrated among the 25 percent of male youths who had a previous arrest. Hence, for most male youths (the 75 percent who reported no previous arrest) the program appeared to have a negligible effect, as was the case for female youths.

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[18] Appendix E examines this issue As discussed there, the impact findings for male youths with a previous arrest differ between the two data sources because earnings data on the treatment group and the control group of male youth arrestees differ between the two data sources

The findings for out-of-school youths in this study are not inconsistent with those from the two existing experimental studies of employment and training programs for out-of-school youths.[19] The first, the youth component of the National Supported Work Demonstration, evaluated an intensive work experience program (Manpower Demonstration Research Corporation, 1980); and the second, JOBSTART, evaluated intensive education, employment, and training services provided through JTPA (Cave and Doolittle, 1991). The Supported Work study found negligible post-program impacts on the earnings of youth participants, most of whom were male. JOBSTART found negligible short-term impacts for female youths and large negative short-term impacts for male youths, mirroring the findings of the present study.

Both JOBSTART and the youth component of Supported Work targeted seriously disadvantaged youths, who make up only a portion of the out-of-school youth population targeted by JTPA Title II-A programs. And Supported Work provided far more intensive services than are typically available from JTPA. Thus, the three studies of employment and training programs for youths focus on different target groups and program services.

Nevertheless, none of these studies indicates that the programs examined were able to improve the earnings prospects of disadvantaged youths; and two of the three studies found that the programs actually reduced the earnings of male youths, at least in the short term. The experimental findings to date are therefore cause for concern.

Implications of the Findings

The National JTPA Study is based on an examination of 16 study sites, which are not a probability sample of all JTPA service delivery areas and which, despite their diversity, may not be representative of the nation. Nevertheless, to the extent that the findings in this report apply to other localities, they have important policy implications.

The study has shown that JTPA Title II-A is helping to raise the earnings of many of its participants, especially adults, but it has also identified several groups for whom the program is having no effect or even adverse effects. In particular, the Title II-A programs studied failed to raise the average earnings of out-of-school youths in general, and they reduced the average earnings of male out-of-school youths who reported having been arrested between their sixteenth birthday and random assignment.

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[19] Although many other employment and training programs for youths have been studied in the past, the findings obtained provide little reliable information because of the methodological problems endemic to the nonexperimental research designs that were used. See the review in Betsey, Hollister, and Papageorgiou (1985).

But although this analysis has identified groups not being adequately served by the program, we cannot use these findings to prescribe ways to serve them better. The study was designed to observe only the impacts of JTPA as it was operated during the study period, not alternative ways of serving the same population.

Finding ways to improve program performance for those groups negligibly or adversely affected by the current program will require experimentation with a range of alternative service strategies for those groups *and rigorous evaluation of their impacts*. We cannot overemphasize the importance of rigorous evaluation of new approaches to serving these groups. Experience has demonstrated that simply trying out alternative program strategies without rigorous evaluation is not enough. As a National Research Council report concluded in reviewing some 400 reports on a wide range of youth employment and training demonstrations, "Despite the magnitude of the resources ostensibly devoted to the objectives of research and demonstration, there is little reliable information on the effectiveness of the programs in solving youth employment problems" (Betsey, Hollister, and Papageorgiou, 1985). To address this deficiency, the authors recommended greater reliance on field experiments with random assignment.

Indeed, the reason it is difficult to draw conclusions from studies that do not use random assignment is clear from our findings on the control groups in this study. The patterns of control group earnings over the 18-month follow-up period demonstrate that even without access to JTPA both adults and youths would have experienced a growth in earnings, and their earnings would have varied substantially across the three service strategies. In other words, if one looks only at the post-program earnings and employment of program *participants*, one can easily mistake patterns of outcomes that would have occurred anyway for *impacts* of the program.

Finally, although the findings presented here clearly reveal a need for some program changes, the full findings of the National JTPA Study have not been obtained. Our forthcoming final report will extend the analysis in several ways. First, we will estimate program impacts on earnings, employment, and educational attainment over a longer follow-up period. Growth or decline in the impacts during the period beyond 18 months could materially alter the differences in estimated impacts among target groups, service strategy subgroups, and other key subgroups that we have observed thus far. Second, we will also include estimated impacts on the receipt of AFDC and food stamp benefits. Third, and most important, we will compare the impacts and costs of JTPA Title II-A and its three service strategies, to determine the cost-effectiveness of the program at the 16 study sites.

1

Background: JTPA Title II-A Nationally, Previous Research, and the National JTPA Study

THE National JTPA Study was commissioned by the U.S. Department of Labor in 1986, in response to a Congressional mandate to study the effectiveness of programs funded by the Job Training Partnership Act of 1982. This JTPA legislation specified that analysis be conducted of the "increase in employment and earnings for participants, reduced support costs, [and] increased tax revenues" (section 454 of the act).

The National JTPA Study employs a *randomized experiment* to estimate the impacts of JTPA Title II-A programs operated by 16 local service delivery areas (SDAs) in the continental United States. Specifically, over a period starting in November 1987 and ending in September 1989, the experiment randomly assigned all Title II-A eligible adults and out-of-school youths who applied to these 16 study sites and were judged appropriate for JTPA by site staff to one of two groups: a *treatment group*, whose members were given access to program services, and a *control group*, whose members were not allowed to receive program services, for a period of 18 months after their random assignment.[1] The study compares the subsequent earnings, employment, and welfare receipt of these two matched groups to obtain estimates of Title II-A impacts on the populations served at the sites.

The decision by the Department of Labor (DOL) to sponsor this type of study was based on a growing consensus among researchers at the time that a randomized experiment was indeed necessary to achieve valid and reliable evidence of the impacts of employment and training programs[2]—and on the unanimous recommendation to the same effect offered by a research advisory panel convened by DOL to determine how best to evaluate JTPA programs (Stromsdorfer et al., 1985).

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[1] The period of random assignment was different for each SDA, but the first sample member entered the study in November 1987 and the last one entered in September 1989.

The 16 SDAs that participated in the study represent a broad range of programs, program participants, and labor markets. All told, the study's findings—based on survey data, SDA administrative records, and data from state unemployment insurance agencies—will provide the first valid and reliable evidence of the impacts of JTPA Title II-A programs. The analysis focuses on a variety of different groups within the study sample of 20,601 eligible program applicants. A first set comprises four main *target groups* of Title II-A: economically disadvantaged adult women and men and female and male out-of-school youths.[3] A second set comprises groups defined by clusters of specific program services, or service strategies, recommended for them by SDA intake staff. The study's analysis of these *service strategy subgroups* offers insight into the impacts of different combinations of specific program services on the groups of program participants deemed likely to benefit from them. Finally, the study also examines impacts on a number of *key subgroups* defined by individual characteristics—such as ethnicity, race, or such barriers to employment as welfare receipt, limited education, and limited recent work experience—that figure prominently in JTPA policy debates.

This report presents estimates of program impacts on the earnings and employment of each of these groups over the 18 months following random assignment. A companion volume (Doolittle, forthcoming) describes how the 16 SDAs operated their Title II-A programs at the time of the study and how the randomized experiment was implemented. The other volumes in this series are listed at the front of this report. Our forthcoming final report will examine impacts on earnings, employment, and welfare receipt over a longer follow-up period and present a benefit-cost analysis of the local programs studied.

The remainder of this chapter offers background on the JTPA program nationally, the results and limitations of previous research on employment and training program impacts, and the more specific goals and objectives of the National JTPA Study.

The JTPA Title II-A Program Nationally

The federal government has sponsored job-training programs for unemployed and economically disadvantaged Americans for almost three decades. These programs began with the

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[2] See Fraker and Maynard (1984); LaLonde (1984); Betsey, Hollister, and Papageorgiou (1985); and Burtless and Orr (1986).

[3] The study excluded in-school youths, for reasons discussed in Chapter 2.

Manpower Development and Training Act of 1962 (MDTA), which was replaced in 1973 by the Comprehensive Employment and Training Act (CETA), which, in turn, was replaced in 1982 by the Job Training Partnership Act (JTPA)—the current federal program. Title II-A of JTPA—the focus of the present study—is designated to serve the employment and training needs of economically disadvantaged adults 22 years of age and older and youths, 16 to 21 years old.[4] According to its statement of purpose (section 2), Title II-A of JTPA is intended:

> to prepare youth and unskilled adults for entry into the labor force and to afford job training to those economically disadvantaged individuals and other individuals facing serious barriers to employment, who are in special need of such training to obtain productive employment.

For adults, the program is intended to increase earnings and employment, and reduce dependence on welfare. For youths, the program has somewhat broader objectives, which include fostering their attainment of educational credentials and occupational competencies, as well as increasing their earnings and employment.

JTPA was one of the first "New Federalism" programs, which sought to decentralize program planning and oversight. As such, it has stimulated wide variation in program content and administration. The ability to tailor programs to local needs and opportunities, rather than to implement a standard intervention, is fundamental to JTPA.

ADMINISTRATION

JTPA Title II-A is funded by the federal government, which spends about $1.8 billion annually to serve roughly a million participants a year (U.S. General Accounting Office, 1991). The states coordinate and regulate local JTPA activities, which are administered by county and city governments.

Within this framework the federal government allocates JTPA funds in two parts. The largest part, 78 percent of the total, is allocated by a formula directly to the local SDAs administering the program.[5] The remaining 22 percent is allocated to the states as set-asides to promote specific program objectives.[6]

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[4] Some local Title II-A programs also serve 14- and 15-year-olds.

[5] The formula allocates the 78 percent of funds in two steps: first to each state, and then to the SDAs within each state. The states, however, have no direct role in this allocation.

[6] These state set-asides are 3 percent for services to older workers, 8 percent to coordinate JTPA programs with educational programs, 6 percent for SDA performance incentives, and 5 percent for state auditing and administrative costs.

Nationally, there are 649 SDAs, covering every part of the country. Formed by one or more local governments, the SDAs operate local JTPA programs with guidance from a Private Industry Council. These PICs comprise representatives of local businesses, unions, social service agencies, and employment and training organizations.

SERVICES

SDAs provide *specific employment and training services* (often termed *program activities*) either directly through their own staff or by contracting with other local service providers, such as public schools, community colleges, proprietary schools, and community-based organizations. The specific services offered come in many different forms, but they generally fall under one of six basic categories:

- *classroom training in occupational skills*, in-class instruction teaching specific job skills, such as word processing, electronics repair, and home health care;
- *on-the-job training*, subsidized training that takes place as part of a paying job, often in a private sector firm (JTPA usually pays half of the wages for the training subsidy up to six months, but the jobs are supposed to be permanent);
- *job search assistance*, assessment of participants' job skills and interests, along with training in job-finding techniques and help in locating job openings;
- *basic education*, including Adult Basic Education (ABE), high school or General Educational Development (GED, or high school equivalency) preparation, and English as a Second Language (ESL);
- *work experience*, temporary entry-level jobs designed to provide basic employment skills and instill effective work habits (the jobs may be subsidized by JTPA if they are in the pubic sector); and
- *miscellaneous services*, including assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among a variety of other services.

For adult and out-of-school youth "terminees" who were enrolled in Title II-A programs nationwide during the sample intake period for the present study (November 1987 to September 1989), the most common *specific* services received were on-the-job training (28 percent of JTPA enrollees), classroom training in occupational skills (28 percent), and job search assistance (25 percent).[7]

PARTICIPANTS

Among the adults and out-of-school youths who were enrolled in Title II-A nationally during the sample intake period for the present study, 95 percent were classified as economically disadvantaged.[8] About 86 percent were identified as facing one or more barriers to employment, including limited education, limited recent work experience, and others.[9]

The adults and out-of-school youths who enrolled in JTPA during this period were 54 percent female and 46 percent male. In terms of their ethnic backgrounds, 54 percent were white, 30 percent were black, and 12 percent were Hispanic. About 65 percent were high school graduates; but 48 percent were receiving some form of public assistance when they applied to JTPA, and 29 percent were receiving Aid to Families with Dependent Children (AFDC).

PERFORMANCE

One distinguishing feature of JTPA is its emphasis on program performance standards, especially with regard to the return on the program's investment in human capital, or the labor market skills and experience of program participants. For example, as stated in section 106(a) of the JTPA legislation:

> The Congress recognizes that job training is an investment in human capital and not an expense. In order to determine whether that investment has been productive, the Congress finds that—

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[7] Job Training Quarterly Survey (JTQS) data. The JTQS is conducted by the U.S. Bureau of the Census, under contract to DOL, and reported by Westat, Inc.

[8] JTPA defines *economically disadvantaged* as having a family income equal to or below the poverty guideline set by the U.S. Office of Management and Budget or 70 percent of the lower living standard set by the U.S. Department of Labor. The data presented in this and the following paragraph were computed from Job Training Quarterly Survey (JTQS) data for the relevant months.

[9] Ten types of barriers to employment were included: (1) having been employed 15 or fewer weeks during the 26 weeks before application to JTPA (67 percent of the enrollees); (2) lack of a high school diploma (35 percent); (3) having reading skills below the seventh grade level (22 percent); (4) being an ex-offender (9 percent); (5) being physically handicapped (9 percent); (6) being a war veteran (9 percent), (7) being a long-term AFDC recipient (9 percent); (8) being over 55 years old (6 percent); (9) having a limited English speaking ability (4 percent); and (10) being a displaced homemaker (3 percent).

(1) it is essential that criteria for measuring the return on this investment be developed; and

(2) the basic return on the investment is to be measured by the increased employment and earnings of participants and the reductions in welfare dependency.

As a result of this emphasis, DOL has expended considerable effort to develop a system of performance standards by which to judge SDAs' achievement of program goals.[10] The standards for adults focus on employment and wage rates, for participants in general and for welfare recipients in particular; those for youths focus on employment and attainment of one or more measures of skills enhancement. DOL also established standards for program costs, but less emphasis has been placed on those standards in the past several years.

Among the adults who entered Title II-A during the sample intake period for the present study, 69 percent had entered an unsubsidized job before leaving the program (that is, before their enrollment was terminated). The average hourly wage for those jobs was $5.86. Among out-of-school youths, 71 percent entered an unsubsidized job, began further training, or achieved another goal defined by DOL as a "positive termination."[11]

These standards measure certain *outcomes* of participating in JTPA Title II-A programs, but they provide no indication of program impacts. For example, the fact that 69 percent of adult terminees found an unsubsidized job does not mean that JTPA caused their employment to occur. It is possible that all of these terminees who found a job might have done so without access to JTPA; if this were true, then we would have to say the program had *no* impact. On the other hand, if very few adult terminees would have found a job without JTPA, then the program had a large impact. In other words, a program outcome measure alone does not allow us to determine what the program actually caused to happen.

To measure JTPA program *impacts*, one must compare the labor market outcomes of program participants with the outcomes they would have experienced without the program—as measured by the experience of a control group whose members did not have access to the program.

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[10] The original Title II-A performance standards measured only immediate post-program outcomes. DOL added several measures of subsequent labor market outcomes in program year 1988.

[11] The findings in this paragraph were computed from JTQS data for a sample of JTPA terminees who were enrolled in the program during the sample intake period for the present study.

RECENT POLICY AMENDMENTS

Because the Job Training Partnership Act was enacted as permanent legislation, it has not been subject to periodic reauthorizations, as CETA was. The JTPA program has therefore had a more stable history than its immediate predecessor, and was already a well-established program when this study began in 1987.

In 1986 Congress instituted minor changes in the program, and in 1988 DOL established new performance standards. On September 7, 1992 President Bush signed the Job Training Reform Amendments of 1992 into law (PL102-367). These amendments to JTPA address the following issues, among others:

- **Program targeting.** In response to concerns that JTPA's emphasis on performance standards discourage SDAs from serving clients who are most in need, the amendments require that at least 65 percent of the adults and youths in the year-round program be persons with identifiable barriers to employment.

- **Program services.** The amendments require a formal objective assessment and an individual service strategy for all program participants. Basic skills and occupational skills training must be provided if the assessment suggests they are needed and work experience or job search assistance may not be provided alone unless the assessment indicates this is appropriate. Furthermore, enrollment in OJT is limited to six months and this period must vary in accord with the level of skills for which training is provided.

- **Program performance.** The amendments specify that incentive grants to SDAs be based in part on the extent to which they serve persons with identifiable barriers to employment. In addition, performance standards must now reflect participants' acquisition of basic skills, achievement of specific occupational competencies or attainment of high school equivalency credential.

- **Programs for youths.** The amendments provide a separate title, II-C, for year-round programs for youths. At least 50 percent of the participants in this new title must be out-of-school youths. The Summer Youth Employment and Training program, Title II-B, is maintained as a separate program.

- **Other issues.** The amendments also restructure current limitations on how SDAs can spend program funds, it modifies the basic formula for allocating JTPA funds to SDAs, it requires procedures to increase the fiscal accountability of SDAs, it specifies improvements to the data collected about local programs, and it includes provisions to enhance the coordination of JTPA programs with other human service programs.

Previous Studies of Employment and Training Programs

Researchers have been trying to measure the impacts of employment and training programs for as long as the programs have been part of federal social policy. Since the passage of MDTA in 1962, literally scores of these studies have been conducted.

THE CENTRAL METHODOLOGICAL PROBLEM

The central methodological problem in all of these studies has been how to determine what the labor market experience of participants would have been without their access to the program in question. The most common approach has been to select a *comparison group* of persons as similar to program participants as possible, but who did not participate in the program. The labor market outcomes of this comparison group have then been used to estimate what the participants' labor market outcomes would have been in the absence of the program. In addition, researchers have used statistical models to adjust their estimates for observed differences between participants (the *treatment group*) and the comparison group.

The problem stems from the fact that the only way to adjust for differences between these two groups is by using individual characteristics that can be measured. Thus, one cannot control directly for characteristics that affect labor market outcomes but that cannot be measured fully, such as motivation. Nevertheless, if these unmeasured or partially measured factors are the same for program participants and comparison group members, on average, or if they correlate in specific ways with factors that can be measured, they can be fully accounted for in estimates of program impacts.

For example, if the motivation level of participants and comparison group members were the same, the effect of this factor on the labor market outcomes of each group would be the same. In this case the unmeasured characteristics of the two groups would balance out and would not bias the estimates of program impacts.[12]

But if unmeasured characteristics that affect labor market outcomes are not well balanced between program participants and the comparison group, the impact estimates produced by comparison group methods will be biased. For example, if the motivation of program participants were higher than that of comparison group members, it would not be appropriate to attribute all of the subsequent difference between the earnings of these two groups to the program being studied. To do so would overstate the actual program impact, because even without the program, the participants would have earned more, on average, than the comparison group members.

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[12] In addition, if the motivation level were different for program participants and comparison group members, but if it were correlated in certain ways with characteristics that were measured (such as past earnings, age, gender, and race), then by controlling statistically for differences in measured variables one could simultaneously control for the differences in unmeasured factors. Once again, the effect of unmeasured characteristics would be "neutralized."

This problem of *selection bias* has been insurmountable for comparison group studies of the impacts of employment and training programs. Although a wide range of sophisticated statistical matching and modeling procedures have been used to address the problem, no acceptable solution has yet been reached.

The basic limitation of the studies is simply that without perfect measures of the unmeasured variables, one cannot be certain whether the selection bias has been removed. In fact, that certainty is possible only when the problem does not exist. Comparison group studies therefore require an assumption that the problem has been resolved by the procedures used to adjust for selection bias. But different procedures have produced different results; and we cannot choose among the procedures with confidence because we cannot know which procedures most successfully removed the selection bias.

Random assignment is an alternative way to choose a group whose experience will reflect what program participants' labor market outcomes would have been without access to the program. Researchers are increasingly using this approach—which relies on a *control group* matched to the treatment group—because of its ability to eliminate selection bias.

Basically, random assignment is like a lottery. Individuals first apply to a program and are screened to ensure their eligibility. Next, much like the flip of a coin, a computer randomly determines who can enter the program and who cannot. If there are more applicants than can be served by the program anyway, this procedure is a fair way to allocate the scarce resources involved. In addition, the laws of probability ensure that the applicants who are denied access to the program (the control group) do not differ systematically from the applicants who are offered access (the treatment group) in any way, measurable or not.

Thus, the subsequent labor market outcomes of control group members serve as valid estimates of what these outcomes would have been for treatment group members if the latter had not had access to the program. And therefore, the difference between the labor market outcomes of the treatment and control groups represents a valid estimate of the true impact of the program.

EARLY STUDIES

The numerous studies of employment and training programs conducted in the 1960s and 1970s were generally limited to measuring short-term post-program earnings and employment, as well as a few demographic characteristics, for program participants and members of a comparison group. Differences in demographic characteristics between the treatment group and comparison group were controlled for using standard statistical methods (ordinary least squares regressions). Because the data and the statistical techniques used to control for selection bias in these studies were inadequate, little systematic knowledge emerged from them (see Perry et al., 1975).

SECOND GENERATION STUDIES

Several studies conducted later in the seventies and early eighties were based on longitudinal earnings data for program participants and comparison group members (Ashenfelter, 1978; Kiefer, 1979; Cooley, McGuire, and Prescott, 1979; and Bloom, 1984b). These and subsequent studies applied relatively sophisticated statistical models to extensive data on large samples.[13]

The basic approach was to adjust for differences in the pre-program earnings patterns of participants and comparison group members when comparing the post-program earnings of the two groups. Here the assumption was that because pre-program earnings predict participants' post-program earnings without the program, controlling for the difference in pre-program earnings between participants and comparison group members would reduce selection bias to an acceptable level.

THE NATIONAL CETA EVALUATIONS

Optimism in the research community about the ability of longitudinal earnings data to control statistically for treatment-control group differences in pre-program earnings and thereby yield valid program impact estimates led to the adoption of this second generation approach as the core strategy for the national CETA evaluations, which began in the 1970s. The evaluations were based on data from the Continuous Longitudinal Manpower Survey (CLMS), the Current Population Survey (CPS), and earnings records maintained by the Social Security Administration (SSA).

The CLMS was a large-scale survey of CETA participants. It collected detailed information on their individual characteristics and linked this information to annual earnings data on sample members in SSA records. The comparison group for the evaluations was drawn from the CPS.

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[13] Ashenfelter (1978) used an autoregressive model; Kiefer (1979), a fixed-effect model; and Bloom (1984b), a time-varying, fixed-effect model.

The CLMS and CPS data made it possible for researchers to combine statistical models of longitudinal earnings with a variety of procedures to match members of the comparison group to CETA participants, based on the detailed data on individual characteristics available for both groups.[14]

Several major studies were commissioned to estimate CETA impacts from the CLMS. Exhibit 1.1 draws from Barnow's (1987) detailed review of these studies. Most striking are the results for male participants, which ranged from estimates of small earnings *gains* to large earnings *losses*, depending on the study. But the results for females also varied substantially; three of the four studies found that CETA markedly increased annual earnings, but the fourth found almost no effect. Thus, for both males and females the estimates of CETA impacts depended critically on the statistical method used.[15] And according to Barnow (1987, 157):

> Data limitations and the inability to adequately test the validity of the selection processes assumed make it impossible to determine which studies modeled the process correctly.

RANDOMIZED EXPERIMENTS

In the mid-1970s researchers began to use an alternative approach, randomized experiments, to measure the impacts of employment and training programs. This approach, as noted earlier, employs a lottery to choose which eligible applicants to a program are allowed to participate (the treatment group) and which are not (the control group). Again, the subsequent labor market outcomes of the control group serve as a valid estimate of what the outcomes of the treatment group would have been without the program; and thus, the treatment-control group *difference* in outcomes is a valid estimate of the program *impact*.

The first major employment and training study to use a randomized experiment was the National Supported Work Demonstration (Manpower Demonstration Research Corporation, 1980). Conducted between 1975 and 1979, the demonstration was a rigorous test of an intensive work experience program for four groups: long-term AFDC recipients, young high school dropouts, ex-addicts, and ex-offenders.

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[14] To select comparison group members, Westat (1984a) used discrete cell-matching, and Dickinson, Johnson, and West (1984), a continuous Mahalanobis nearest-neighbor matching procedure. Bassi (1983) and Bloom and McLaughlin (1982) used a simple screening criterion. These authors also produced other reports on their CETA evaluations, which were reviewed in Barnow (1987).

[15] The ranges of impact estimates presented in Exhibit 1.1 for *a given study* (row) reflect findings for different subgroups and thus are not shown here as evidence of a method-specific variation in impact findings. That evidence lies across the *different studies*, that is, in each column in the exhibit.

Exhibit 1.1 *Summary of Estimated CETA Impacts on Annual Earnings, from Four Studies Using the Continuous Longitudinal Manpower Survey (CLMS)*

Study (year published)	*Impact, in $*			
	Adult women		*Adult men*	
Bloom and McLaughlin (1982)[a]	800** to 1,300**		200	
Dickinson, Johnson, and West (1984)[b]	13		-690**	
	White females	*Minority females*	*White males*	*Minority males*
Bassi (1983)[c]	740** to 778**	426** to 671**	n/a	117 to 211
Westat, Inc. (1984)[d]	408** to 534**	336** to 762**	-4 to 500**	-104 to 658**

Source: Barnow (1987, 182-83, table 3).

a. Sample members were ages 25-60; impacts were for program years 1976-1978, converted to 1980 dollars.

b. Sample members were ages 22-64; impacts were for program year 1978, reported in nominal dollars.

c. Sample members were ages 23-60; impacts were for program years 1977-1978, reported in nominal dollars.

d. Sample members were ages 14-60; impacts were for program years 1977-1978, reported in nominal dollars.

** Statistically significant at the .05 level (two-tailed test).

The Supported Work Demonstration found large earnings impacts for AFDC recipients and small to negligible effects for the other three groups. But its successful use of a multisite, randomized experiment to measure the impacts of employment and training programs was an important finding in and of itself, one that would set a methodological precedent for later research.

As the desirability and feasibility of randomized experiments became more apparent, researchers began to use the approach more often. Several experimental studies of employment and training programs were initiated during the early and mid-1980s; some are now completed, while others are ongoing.

Adults. Most of the studies of employment and training programs for adults focused on programs for welfare recipients,[16] although several others examined programs for displaced workers—persons who permanently lost well-paying, stable jobs because of foreign competition or changing technology.[17]

The largest randomized experimental study of employment and training programs to date is the Demonstration of State Work/Welfare Initiatives (Gueron and Pauly, 1991). Begun in 1982, this project tested a wide range of programs for welfare applicants and recipients in eight states, with a total experimental sample of over 45,000 persons. Some of the programs studied covered a broad cross-section of the AFDC caseload, were mandatory for AFDC recipients, and were operated as part of the existing Work Incentive program (WIN). Others covered only selected portions of the AFDC caseload, were voluntary, and were run as demonstrations to investigate the impacts of specific types of services.

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[16] See Gueron and Pauly (1991) for a comprehensive review of these studies.

[17] See Bloom (1990) and Corson et al. (1989).

Work/Welfare provided a wealth of information about the programs' administration, participation rates, costs, and impacts. The study also demonstrated the feasibility of implementing a rigorous randomized experiment at a very large scale and in many sites simultaneously.

Other major randomized experiments studying programs for welfare recipients include the AFDC Homemaker-Home Health Aide Demonstrations, conducted in seven states, with a 9,500-person research sample (Enns, Bell, and Flanagan, 1987); the Louisville WIN Laboratory Experiments, conducted at two local WIN offices, with a 4,200-person sample (Goldman, 1981); the Saturation Work Initiative Model (SWIM), conducted in San Diego, with a 4,600-person sample (Hamilton and Friedlander, 1989); and the already-mentioned Supported Work Demonstration, which had a component for welfare recipients in 10 sites, with a 1,400-person sample.

From a detailed review of the findings from these studies, Gueron and Pauly (1991, 26) concluded that:

> Almost all of the welfare-to-work programs studied led to earnings gains. This was true for both low- and higher-cost programs and services, and for broad-coverage and selective-voluntary programs....Seven of the nine broad-coverage programs led to increases in average annual earnings, ranging from $268 to $658 in the last year of follow-up. Depending on the program, this was 11 to 43 percent above the annual earnings of people in the control group. The smaller-scale, selective-voluntary programs increased average annual earnings by $591 to $1,121—14 to 34 percent above the control group's earnings.

The authors further concluded that:

> Earnings impacts for both low-cost job search and higher-cost programs were sustained for at least three years after program enrollment.

Experimental studies of programs for displaced workers also provide a useful point of reference, because they include adult men, who are not well represented in the other randomized experiments.[18] Two studies are particularly relevant to the present one: the Texas

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[18] Exceptions are the findings for ex-addicts and ex-convicts in the Supported Work Demonstration, almost all of whom were men, and the findings on several Work/Welfare programs that served men receiving AFDC-UP.

Worker Adjustment Demonstration (Bloom, 1990), a three-site, 2,200-person study; and the New Jersey Unemployment Insurance Reemployment Demonstration Project (Corson et al., 1989), a 10-site, 11,100-person study.[19] The programs, target groups, and economic environments examined were quite different in each, but both studies suggest that employment and training programs can increase the earnings and employment of displaced workers. In addition, the Texas study indicates that program impacts were larger and more sustained for women than for men (Bloom, 1990, vii).

Youths. The best existing information on the impacts of employment and training programs for youths is from the youth component of the National Supported Work Demonstration and the recent JOBSTART demonstration.

As described earlier, Supported Work tested an intensive work experience program for four groups, one of which was a group of about 900 young high school dropouts, most of whom were male, located in five sites. Findings from the study indicated negligible impacts on post-program earnings or employment for those youths (Manpower Demonstration Research Corporation, 1980).

JOBSTART is an ongoing study of 2,200 young high school dropouts in 13 sites. Interim findings suggest a minimal post-program impact on the earnings of female youths and a *negative* impact on male youths. Specifically, during the second year after random assignment the impact on young men was $-667, or 13 percent *less* than what they would have earned without access to the program. Additional follow-up is under way to determine whether this negative impact persisted for male youths and whether the impact for female youths rose or declined over time.

METHODOLOGICAL STUDIES OF EXPERIMENTAL AND COMPARISON GROUP TECHNIQUES

While the findings from the national CETA evaluations were becoming available, a series of studies was conducted to examine the methodological properties of experimental methods and nonexperimental comparison group methods. Fraker and Maynard (1984) and LaLonde

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[19] The Buffalo Dislocated Worker Demonstration Program (Corson, Long, and Maynard, 1985) is another example. Although conducted as a randomized experiment, the study estimated program impacts using nonexperimental comparison group methods. Thus, its findings are not directly comparable to those of the Texas and New Jersey demonstrations.

(1984) used data from the Supported Work Demonstration to test the ability of comparison group methods to emulate the findings from the experiment. Ashenfelter and Card (1985) used CLMS data to explore the variation in findings from different comparison group methods applied to the same data. The three studies reached the same basic conclusion. As Fraker and Maynard reported (1987, 216, 220):

> The overwhelming conclusion from this study is that comparison group study designs should be avoided when reliable estimates of program impacts are an important study objective....
>
> For the time being the safest evaluation strategy involves the use of a true control group.

RECOMMENDATIONS OF THE NATIONAL ACADEMY OF SCIENCES COMMITTEE ON YOUTH EMPLOYMENT PROGRAMS

In 1985 the National Academy of Sciences convened a committee to review the existing research on employment and training programs for youths, especially those funded through the Youth Employment Demonstration Projects Act (YEDPA). The committee found that little could be said with confidence about the impacts of programs for youths, because the comparison group strategies that had been used to study the programs did not offer convincing evidence. The committee also concluded that (Betsey, Hollister, and Papageorgiou, 1985, 18, 30):

> control groups created by random assignment yield research findings about employment and training programs that are far less biased than results based on any other method....
>
> Future advances in field research on the efficacy of employment and training programs will require a more conscious commitment to research strategies using random assignment.

RECOMMENDATIONS OF THE JTLS RESEARCH ADVISORY PANEL

Soon after JTPA was authorized in 1982, the Department of Labor began plans for a national evaluation of the program. This evaluation was to build on the longitudinal comparison group approach used in the CETA evaluations. It was to include a detailed survey for a national sample of JTPA participants, referred to as the Job Training Longitudinal Survey (JTLS) and a special national survey, the Survey of History of Work (SHOW) for constructing a comparison group (Westat, 1984b).

But when the inconsistent findings from the various CETA studies began to emerge, and some of the early findings from the methodological studies of experimental and comparison group techniques were becoming available, DOL staff members began to rethink the Department's plans. Seeking guidance on this issue, DOL convened a panel of experts; and authors of the CETA studies were invited to present their findings and recommendations to the panel. The panel concluded (Stromsdorfer et al., 1985, 21):

> The recommendations of the panel are strongly conditioned by the judgment that it will not be possible to solve the problem of selection bias within the context of a quasi-experimental design such as the JTLS/ SHOW, at least not in a short enough time frame to meet Congress' needs for valid information to guide policy. Even though many authors studying employment and training programs have recognized the selection problem, no such study using a quasi-experimental design can be said to have controlled adequately for selection bias. The panel does not intend to set forth a counsel of despair. Rather, it is concerned that the past evaluations of CETA have consumed, and the contemplated evaluations of JTPA will continue to consume, millions of dollars and much valuable time. It would be extremely unfortunate if the analysis of the JTLS/SHOW design would yield the same ambiguous conclusions as has the analysis of the CLMS/CPS database for CETA.

There were also well-acknowledged trade-offs with the alternative: a randomized experiment. On the one hand, the panelists understood that the experimental approach represented the best chance to obtain valid and reliable impact estimates for the local programs to be studied. On the other hand, they recognized that not all local programs would agree to participate in such a study, and thus it would be difficult to obtain a probability sample of sites to ensure the generalizability of findings to the JTPA program nationally.

On balance, then, the advisory group decided that without valid estimates for the sites in the study, the issue of generalizability was not relevant. Its recommendation was therefore (p. 22):

> The DOL should perform a selected set of classical experiments over the next several years that involve random assignment of program-eligible individuals to the treatment (experimental) group and to the non-treatment (control) group. This is the key recommendation of the panel. The intent is to use these experiments to:
>
> - evaluate the net impact of JTPA for selected target/ treatment groups in a set of SDAs that volunteer to participate.

- use these experimental results and the understanding of the selection process gained thereby to improve the effectiveness of quasi-experimental designs as a strategy for program evaluation.

The National JTPA Study in Brief

In June 1986 DOL awarded two separate contracts to conduct the National JTPA Study:

- a *Part A* contract with the Manpower Demonstration Research Corporation (MDRC) and its subcontractors, the National Governors' Association, the National Association of Counties, and the National Alliance of Business, to implement and monitor the experiment; and
- a *Part B* contract with Abt Associates Inc. and its subcontractors, New York University, MDRC, the National Opinion Research Center (NORC), Fu Associates, and ICF, Inc., to design the study, collect the required data, and conduct the analyses.

Based on the recommendations of the JTLS Research Advisory Panel, the National JTPA Study consists of two parts:

- a *randomized experimental study* of JTPA Title II-A programs, which is based on the experiences of 20,601 eligible adults and out-of-school youths who applied to 16 local SDAs in the continental United States between November 1987 and September 1989; and
- a *nonexperimental methods study* to develop new comparison group procedures for estimating program impacts.

The core of the study is the randomized experiment, in which eligible program applicants were randomly assigned to either a treatment group, whose members were offered access to Title II-A services, or a control group, whose members could not obtain those services for a period of 18 months. (The control group could, however, obtain employment and training services from other local programs.) As demonstrated in Bloom (1991) and Appendix A here, the treatment and control groups were indeed well matched, as one would expect from a strictly applied random assignment procedure.

As noted in the introduction to this chapter, because of the large sample size (20,601), the study is able to make valid treatment-control group comparisons for a variety of different subgroups, including four main target groups, groups recommended for different clusters of services, and selected key subgroups of interest to policymakers and program planners.

The primary goal of the National JTPA Study—to estimate the effectiveness of Title II-A programs as they normally operate—called for certain key decisions on the study's design.[20]

First was the challenge of recruiting and selecting SDAs to serve as sites. Because the study did not have a legislative mandate that required SDA participation, it had to rely on SDAs that were willing to volunteer. But SDAs were concerned about participating in the study for a number of reasons and hence were reluctant to participate.

For example, the experimental design required to address the key research questions of the study was complex, and SDAs were concerned about its possible effects on their programs. In addition, SDAs were concerned about the potential political fallout that random assignment might generate. Further complicating matters was the fact that in order for an SDA to participate, all local organizations and key individuals involved (the SDA, its PIC, the vendors, and local government officials) had to agree. This requirement of unanimity greatly reduced the chances that a prospective site would volunteer.

For these reasons, it was not possible to draw a strict probability sample of sites.[21] Instead, a range of SDAs from across the country were recruited to participate. And from among them, the 16 SDAs that were willing and able to participate became sites for the study (see Doolittle and Traeger, 1990).

Second, because JTPA program staff often recommend more than one program service for an applicant, the study was designed to measure the impacts of clusters of program services—what we term *service strategies*—not single services in isolation, such as classroom training in occupational skills, or on-the-job training, or job search assistance. Isolating the impacts of single services would require comparing the experiences of treatment and control group members for each. But to construct such treatment and control groups would require a special demonstration that would have to be run quite differently from JTPA programs.

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[20] See Bloom et al. (1990) and Doolittle and Traeger (1990).

[21] Original plans called for a probability sample of sites, although the difficulty of achieving this objective was acknowledged from the outset. When it became clear that this approach was not feasible, given the constraints of the present study, the process was modified to one that focused on recruiting the most diverse group possible of SDAs that were willing and able to participate.

Instead, this study was designed to estimate the impacts of three distinct service strategies: one that recommended sample members for classroom training in occupational skills (and in some cases other, secondary services); a second that recommended sample members for on-the-job training (and in some cases other, secondary services); and a third that recommended sample members mainly for other services besides classroom training in occupational skills and OJT. The mix of services sample members actually received was distinctly different for each services strategy and reflected in part (but not entirely) the main service for which the sample members were recommended.

Third, because control group members would be able to receive employment and training services from other, non-JTPA providers, the study was designed to estimate the effect of JTPA as an *incremental source* of these services. This is probably the most relevant comparison to make, because JTPA expenditures add services to the existing landscape of employment and training programs. Thus, to assess the program in this regard requires examining the extent to which JTPA adds services to the local community and, in turn, the extent to which this increment in services resulted in an impact on labor market outcomes for the treatment group. Our forthcoming final report will also compare the incremental *costs* of adding JTPA services and the incremental impacts of the program, to determine the cost-effectiveness of the local programs studied.

Finally, because local JTPA program staff can only offer program services to applicants, not force them to enroll, the study is designed to provide estimates of the impact of offering *access* to JTPA services, not the impact of receiving them. From these estimates it is also possible, however, to infer what the likely impact of receiving program services was. The study will provide these inferred estimates as an additional perspective from which to judge the program's effectiveness; but less confidence can be placed in these estimates because they are inferred indirectly.

Summary

The National JTPA Study offers important substantive and methodological contributions to the literature, especially in light of how little is known about the effectiveness of employment and training programs, and how to measure their effectiveness.

The study will provide valid and reliable evidence on the effectiveness of JTPA Title II-A programs in a diverse group of sites. It will identify whom those programs are working for (or not) and which services strategies are working (or not) for each target group. By identifying program successes, the study can help guide future efforts to study the factors that promote success. And by identifying situations in which the program is not working, the study can help target efforts for change.

But the study findings by themselves cannot provide a blueprint for action. They can only identify issues to be addressed in the future, which must, in turn, be based on the development and rigorous testing of new approaches to serving the labor market needs of disadvantaged persons.

2

Preview: The 18-Month Impact Analysis

THIS chapter describes the 18-month impact analysis of the National JTPA Study. The first main section outlines the implementation of the experimental design, indicating how the 16 study sites were selected and how client intake and random assignment were conducted. This section also defines the four target groups and three service strategy subgroups for which program impacts were estimated.

The second main section describes the types of program impacts estimated in the 18-month analysis. The section begins by defining the 18-month study sample and then distinguishes between impacts on the treatment group overall *(impacts per JTPA assignee)*, which were estimated directly from the experimental data, and impacts on those treatment group members who were actually enrolled in the program *(impacts per JTPA enrollee)*, which were inferred using a simple extension of the experimental data. We then explain how the impact estimates represent the impact of the *increment* in employment and training services that Title II-A provides, beyond those otherwise available to low-income Americans. The section ends by defining the educational attainment and labor market outcomes used as the basis for measuring program impacts in this report.[1]

The last section describes the five sources of data employed in this report: a Background Information Form completed by sample members when they applied to JTPA; our First Follow-up Survey; enrollment and tracking data from the 16 sites; quarterly earnings data from state unemployment insurance agencies; and the Job Training Quarterly Survey of JTPA participants nationwide.

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[1] Our forthcoming final report will provide estimates of these impacts over a longer follow-up period, as well as estimated impacts on welfare receipt and a benefit-cost analysis.

Implementation of the Study Design

As explained in Chapter 1, the primary goal of the National JTPA Study is to provide valid estimates of the impacts of JTPA Title II-A programs as they were being run at the time of the study in a broad range of different sites. To produce valid impact estimates, the study was implemented as a classical randomized experiment, with matched treatment and control groups.

This section briefly describes the selection of sites for the study and then describes the random assignment process that created the treatment and control groups. We also explain our focus on four main target groups, defined by age and gender, and three main service strategies, defined by the specific program services SDA staff recommended for members of the study sample. Because of the size of the experimental sample for this 18-month analysis (17,026) and the nature of the random assignment, we have in essence separate experiments for each of these main subgroups, as well as for smaller key subgroups defined by such characteristics as ethnicity and the barriers to employment sample members were facing when they applied to JTPA.

SITE SELECTION

As noted in Chapter 1, the 16 study sites were recruited from among service delivery areas (SDAs) in the continental United States.[2] As described in Chapter 3, and in Doolittle (forthcoming), these SDAs represent a broad range of different administrative arrangements, program services, participant characteristics, and labor market conditions. The program impacts reported here therefore reflect much of the diversity that exists within JTPA nationwide.

The sites do not, however, represent a probability sample of SDAs that would allow us to generalize the study findings to the Title II-A program nationwide. For reasons detailed in the final design report (Bloom, Orr, Doolittle, Hotz and Barnow, 1990) and in the first implementation report (Doolittle and Traeger, 1990), it was not possible to recruit such a sample. Instead, we recruited SDAs based on the following criteria: their diversity, their willingness to participate, their ability to implement the experimental design, the size of the experimental sample they could provide, and the likely composition of this sample.

Diversity was a key criterion because of our desire to provide estimates of program impacts under as broad a range of conditions as possible. We did not want to base the study on a few isolated SDAs that were similar to one another and different from most others. And we particularly did not want a sample of SDAs that were unusually successful or unusually unsuccessful in terms of the JTPA standards used to assess their performance.

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[2] In JTPA parlance "service delivery area" refers to both the local administrative agency for the program and the geographic area it serves.

SDAs' willingness to participate was essential because there was no legislative mandate that required them to do so. Moreover, administrators' concerns about problems that might arise if they did were a major obstacle to overcome. One problem that was especially acute was that all of the parties affiliated with the SDA (the SDA director and staff, members of the Private Industry Council, local service providers, and local government officials) had to agree before it could enter the study and properly implement study procedures.

SDAs' ability to implement the fairly complex experimental design, without unduly disrupting their normal operations, was also essential. We therefore did not recruit some SDAs that might have been willing to join the study but were experiencing administrative difficulties.

The size of the experimental sample each SDA could provide was another important consideration. Not only did we need a large total experimental sample, but we also had to limit the number of sites, for logistical reasons. We therefore did not recruit sites with fewer than 500 Title II-A terminees in program year 1984 (the most recent year for which data were available at the time).

Finally, we also took into account the composition of the potential sample at each SDA, to help insure that the sample represented a broad mix of program participants.

The preceding criteria were not embodied in formal site selection rules. Instead, they served as an informal guide to help direct the marketing and outreach efforts of the implementation team.

Exhibit 2.1 shows the names and locations of the 16 SDAs that ultimately participated as sites in the study. In each SDA the experiment included virtually all of the eligible adults and out-of-school youths who applied to JTPA Title II-A during the sample intake period for that SDA and who were judged by SDA staff to be appropriate for program services.[3] That period differed for each SDA, but the first sample member entered the study in November 1987 and the last one entered in September 1989.

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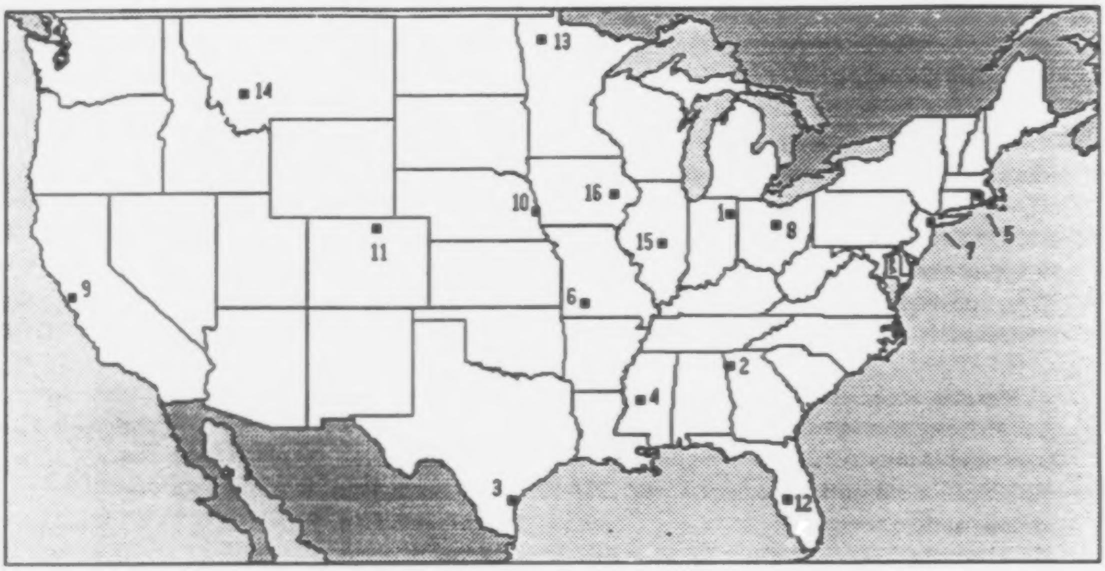
[3] Formal agreements with some of the SDAs excluded certain small groups of applicants from the study (and therefore from random assignment) for one of three main reasons: (1) logistical problems, such as widely dispersed groups that would have required many different intake locations; (2) recruitment problems for particular groups, such as older workers; and (3) the nonvoluntary nature of certain applications, namely, among groups required to apply to JTPA either by the courts (usually as a condition for parole) or as a condition for receiving public assistance. Doolittle and Traeger (1990) describe the groups that were excluded from the experiment, if any, at each site.

Exhibit 2.1 Location of the 16 Study Sites



Key:

1. Fort Wayne, Indiana
2. Coosa Valley, Georgia
3. Corpus Christi, Texas
4. Jackson, Mississippi
5. Providence, Rhode Island
6. Springfield, Missouri
7. Jersey City, New Jersey
8. Marion, Ohio
9. Oakland, California
10. Omaha, Nebraska
11. Larimer County, Colorado
12. Heartland, Florida
13. Northwest Minnesota
14. Butte, Montana
15. Decatur, Illinois
16. Cedar Rapids, Iowa

Notes: The study sites are listed in descending order by the size of the 18-month study sample at each site. For the full name of each service delivery area, its largest city, and its sample size, see Exhibit 3.1 in Chapter 3.

CLIENT INTAKE AND RANDOM ASSIGNMENT

Exhibit 2.2 illustrates the client intake and random assignment process used to create the treatment and control groups. Although specific details of the process varied from site to site, to accommodate existing local institutional arrangements and operating practices, the same basic procedure was followed at all sites.

The process began with normal JTPA procedures for recruiting applicants and determining their eligibility for the program.[4] Those applicants who were judged to be eligible were then assessed by local SDA staff members to determine which JTPA services would be most appropriate for meeting their individual needs. At that point the staff members recommended applicants for one or more specific program services.[5] Those recommendations, in turn, formed the basis for assigning all sample members to one of three service strategy subgroups: classroom training, OJT/JSA, and other services (defined in a later subsection).

During this process staff members explained to applicants that not all of them could be served, and because the SDA was participating in a special study, a lottery would be used to select those who would be allowed to participate in JTPA and those who would not over the next 18 months. Applicants then signed a consent form to indicate that they understood the nature of the participant selection process and to allow the research contractor to obtain information on their earnings, employment, and welfare receipt from the administrative records of governmental agencies.

At that point an SDA staff member telephoned a random assignment clerk from the study team, who randomly assigned each applicant to treatment or control status *within* each service strategy subgroup, as shown in Exhibit 2.2. Specifically, two-thirds of the experimental sample was assigned to the treatment group (whose members were allowed to receive JTPA Title II-A services), and one-third was assigned to the control group (whose members were not allowed to receive those services for the experiment's embargo period of 18 months).[6]

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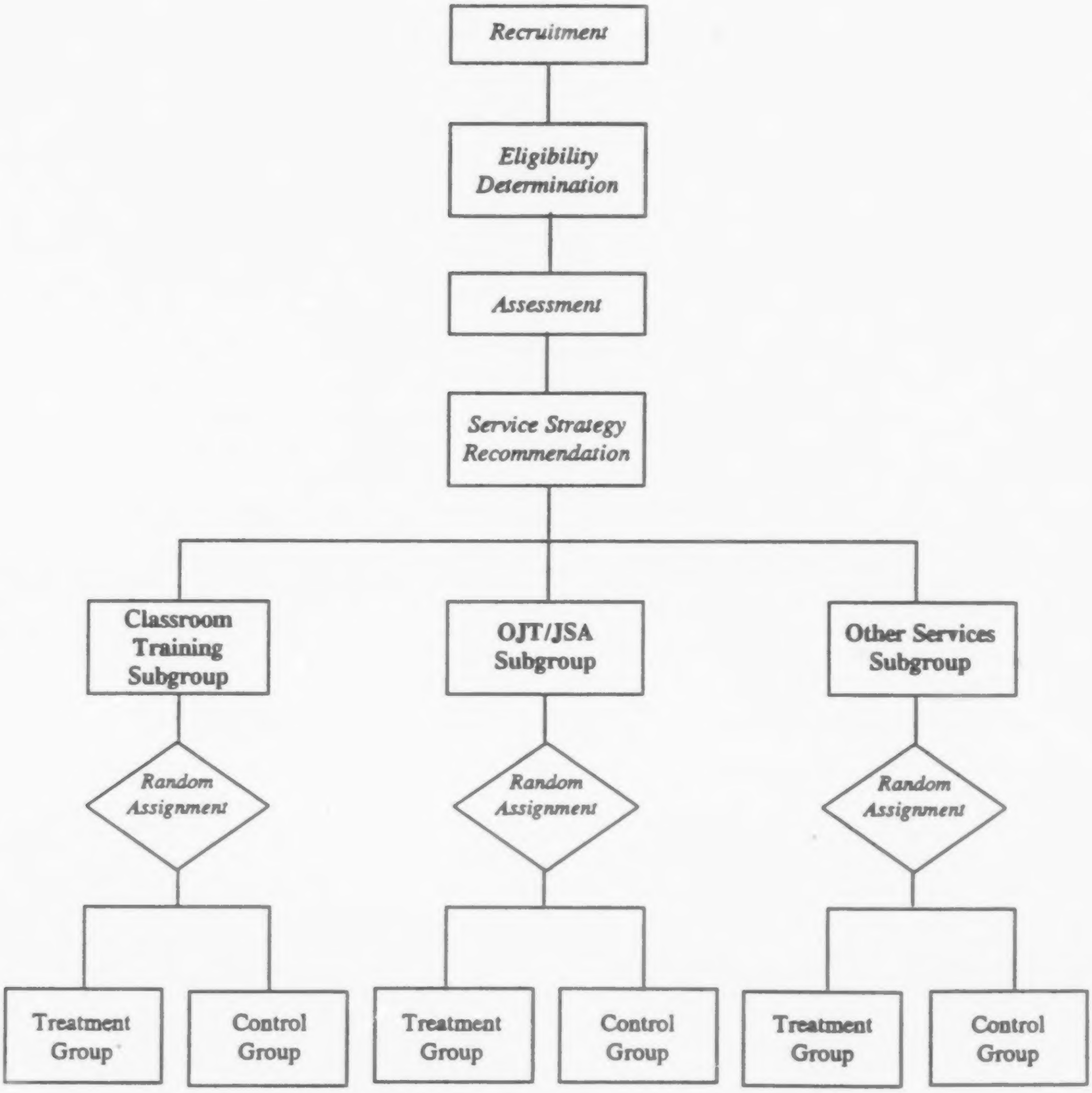
[4] Sites were given a limited amount of technical assistance to improve their client recruitment procedures, so that enough eligible people would apply to JTPA to provide for a control group without reducing the number of persons served by the SDA. To the extent that this additional recruitment changed the mix of clients in the programs, and to the extent that any such change in client mix produced a change in average impacts, the additional recruitment may have altered the nature of the population for which the impact findings can be generalized. There is no empirical evidence with which to assess this possibility, however.

[5] Throughout this report we refer to classroom training in occupational skills, basic education, on-the-job training, job search assistance, work experience, and miscellaneous services as *specific program services*. They are often referred to as *program activities* in the employment and training literature.

[6] This 2/1 ratio of treatment group members to control group members represents an explicit trade-off between the need for statistical precision in program impact estimates (the optimum ratio for which is 1/1) and a practical need to minimize the size of the control group in order to minimize the number of persons that had

Exhibit 2.2 Random Assignment Model for the National JTPA Study



The SDA staff then called or wrote treatment group members to schedule their participation in JTPA; control group members usually were informed of their status by letter, although some were informed by telephone or in person.

THE FOUR TARGET GROUPS

From the outset the study was designed to focus on three main *target groups:* adult women, adult men, and out-of-school youths. Because members of each target group were randomly assigned to treatment or control status independently of one another, the random assignment process produced an independent randomized experiment for each of these target groups.

The distinction between adult women and men is based on the accumulated evidence of differences between the impacts of employment and training programs estimated for the two groups (for example, Ashenfelter, 1978; Kiefer, 1979; Bassi, 1983; Bloom and McLaughlin, 1982; Westat, 1984a; and Dickinson, Johnson, and West, 1984). Out-of-school youths were separated from adults in the analysis because of the major differences between their positions in the labor market. In addition, findings for out-of-school youths are reported here separately for female youths and male youths because the observed impacts appear to differ by gender (see Chapter 6).[7]

The study was limited to out-of-school youths in Title II-A rather than all Title II-A youths, because the evaluation team expected that programs and relevant outcomes for in-school youths and out-of-school youths would differ too much for them to be analyzed together and because the samples for in-school youths were expected to be too small for separate analyses. We also anticipated that it would be difficult to obtain consent to implement random assignment in public schools, where some JTPA services for in-school youths are provided.

In the JTPA Title II-A program nationwide 30 percent of the participants are adult women, 25 percent are adult men, 23 percent are out-of-school youths, and 22 percent are in-school youths. The National JTPA Study therefore focuses on target groups that make up about three quarters of the population currently being served by JTPA.

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to be turned away by local program staff and the number of additional applicants that had to be recruited to provide for a control group.

[7] Original plans were for impacts on youths to be analyzed separately for white and minority youths. But estimated impacts differed less between white and minority youths than between female and male youths (see Chapter 6). Hence, the impact analysis for youths focuses on this latter distinction.

THE THREE SERVICE STRATEGIES

The program services that JTPA applicants ultimately receive depend on a number of factors, including the types of services that the applicants want, judgments by program staff about the suitability of specific services for particular applicants, and the availability of certain services at the time an individual applies to the program. The specific program service or services provided to an applicant are sometimes determined by deliberate planning (for example, a basic education course followed by occupational skills training). At other times, however, they are determined by trial and error, producing a sequence of services that evolves from a continuing effort to find one or more that are suitable.

In short, it is difficult to predict which service or services an applicant will receive. And JTPA often provides more than one service to an applicant. As a result, it was not possible both to achieve our mandate to examine the impact of JTPA programs as they were being operated at the time (Bloom, Orr, Doolittle, Hotz, and Barnow, 1990) and to isolate the effect of receiving a particular program service, because to isolate the effect of a *single* specific program service would require restricting certain sample members to that service, substantially altering the normal decision-making process of JTPA.

Thus, to examine the impacts of the different types of services offered by JTPA programs, we grouped treatment and control group members into three *service strategy subgroups* defined in terms of the specific program services *recommended* for them before random assignment.[8] We based our definitions on service recommendations because (1) doing so made it possible to match treatment group members and their control group counterparts (which would not have been possible using program services *received)*; (2) we judged that service recommendations were the best available predictors of services received and therefore the best available way to distinguish among sample members according to the services they did subsequently receive; (3) this approach had a minimal effect on the normal JTPA decision-making process, and (4) it made it possible to account for the combinations and sequences of services received by many JTPA participants.

Exhibit 2.3 shows the services allowed in the definition of each service strategy. The definitions are based on the initial staff recommendation for each sample member, with modest

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[8] Previous reports for this project (Bloom, Orr, Doolittle, Hotz, and Barnow, 1990; Doolittle and Traeger, 1990; and Bloom, 1991) termed the service strategies "treatment streams" and termed the three strategies as classroom training, on-the-job training, and other activities. The names and characterizations of these service strategies evolved over time as we learned more about the actual services received by each service strategy subgroup.

Exhibit 2.3 Specific Program Services Allowed in Each of the Three Service Strategy Definitions

	Service strategy		
Specific program service	*Classroom training*	*OJT/ JSA*	*Other services*
Classroom training in occupational skills	Yes	No	Yes
On-the-job training	No	Yes	Yes
Job search assistance	Yes	Yes	Yes
Basic education	Yes	Yes	Yes
Work experience	Yes	Yes	Yes
Miscellaneous	Yes	Yes	Yes

restrictions on the specific program services that participants could subsequently receive. Specifically:[9]

- The *classroom training strategy* was defined to include sample members who were recommended for classroom occupational skills training but *not* for on-the-job training (OJT). Any other service—such as job search assistance, basic education, and work experience (but not OJT)—could be recommended in addition to the defining service for this strategy. Most sample members recommended for this service strategy who were subsequently enrolled in JTPA received classroom training in occupational skills or basic education or both (see Exhibit 3.19 in Chapter 3).

- The *OJT/JSA strategy* was defined to include sample members who were recommended for OJT but *not* classroom occupational skills training. All secondary services (but not classroom occupational skills training) could be recommended in addition to the defining service for this strategy. Most sample members recommended for this service strategy who were later enrolled in JTPA received OJT or job search assistance, or both (see Exhibit 3.19).

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[9] Two infrequent exceptions to the service strategy definitions presented here were limited classroom training provided to some members of the OJT/JSA subgroup before they received on-the-job training and limited on-the-job training provided after some members of the classroom training subgroup received classroom training in occupational skills.

- The *other services strategy* was defined to include sample members who were recommended for *neither* classroom occupational skills training nor OJT as the defining service.[10] This strategy produced a substantially different mix of services for adults than for youths. Adults recommended for this strategy who were later enrolled in JTPA received mainly job search assistance and miscellaneous services, such as customized combinations of classroom occupational skills training and OJT (see Exhibit 3.19). Youths recommended for the strategy who became enrolled in JTPA received mainly basic education or miscellaneous services, such as tryout employment (in which participants are hired on a probationary basis to learn a job and prove themselves qualified for permanent employment) and job shadowing (in which participants follow and observe a regular employee to learn what is required to hold a job: see Exhibit 3.19). Hence, for adults this strategy focused more on immediate employment, whereas for youths it focused more on education and entry-level job skills.

As Chapter 3 will demonstrate, these definitions produced subgroups that did, in fact, receive distinctly different sets of program services. Note, however, that each service strategy subgroup ultimately received predominantly two key services. Hence, the study findings reflect more than the impact of the single defining service for each strategy.

Impact Estimates in the 18-Month Analysis

This section briefly describes how we obtained the impact estimates presented in this report, detailing, first, the 18-month study sample upon which these findings are based; then, how we estimated impacts per JTPA assignee (treatment group member) and impacts per JTPA enrollee (treatment group member who actually enrolled in the program); and finally, how we measured the attainment of a training-related high school diploma, earnings, employment, and the components of earnings—the outcomes of interest in this analysis.

THE 18-MONTH STUDY SAMPLE

The random assignment process described above produced a total experimental sample of 20,601 treatment group and control group members from the 16 study sites. Bloom (1991) describes the baseline characteristics of this sample.[11]

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[10] Formal agreements were made with each SDA to specify a maximum allowable percentage of experimental sample members recommended for the other services strategy. This limit was based on the previous experience of each site, and no site reached its limit.

[11] Bloom (1991) describes an experimental sample containing 20,602 cases. Subsequently, two of these cases were discovered to represent the same person.

Exhibit 2.4 Sample Sizes in the 18-Month Study: Full Sample and Target Groups, by Service Strategy Subgroup

Service strategy subgroup	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths*[a] *(4)*	*Male youths*[a] *(5)*
Classroom training	6,113	2,927	1,353	1,193	640
OJT/JSA	6,410	2,322	2,754	612	722
Other services	4,503	1,358	1,519	844	782
All subgroups	17,026	6,607	5,626	2,649	2,144

Source: Unadjusted frequencies based on Background Information Form responses.
a. Out-of-school youths only.

The present report on 18-month impacts is based on a 17,026-person subsample that includes all members of the experimental sample who were scheduled for a First Follow-up Survey interview (described later in this chapter) at least 18 months after their random assignment. We refer to this subsample as the *18-month study sample.*[12]

Because sample members' scheduled interview dates were independent of whether they were assigned to the treatment group or the control group, the 18-month study sample is fully experimental. Hence, the treatment group and the control group for this sample had no systematic differences at random assignment, and their measured baseline characteristics were virtually identical as shown in Appendix A.

Exhibit 2.4 shows the size of each target group and service strategy subgroup in the 18-month study sample. By target group the sample includes 6,607 adult women, 5,626 adult men, 2,649 female youths, and 2,144 male youths. By service strategy subgroup it includes 6,113 persons recommended for the classroom training strategy; 6,410 persons recommended for the OJT/JSA strategy, and 4,503 persons recommended for the other services strategy. For experimental purposes we therefore have large samples for each target group and for each service strategy subgroup. For adult women and men the sample sizes for the service strategy subgroups were also quite large, ranging from 1,353 to 2,927 persons; for female and male youths these subgroup sample sizes were smaller but still substantial, ranging from 612 to 1,193 persons.

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[12] In five sites that experienced recruitment problems the treatment-control group ratio was increased temporarily from 2/1 to 3/1 or 6/1. This reduced the number of eligible applicants lost to the program because they were assigned to control group status. Consequently, the overall treatment-control group ratio for the full experimental sample is slightly greater than 2/1. When constructing the 18-month study sample, however, we randomly deleted these "extra" treatment group members, thus producing an analysis sample with a constant 2/1 ratio for all sites and subgroups. This was done to simplify the corresponding estimates of program impacts, as discussed in Appendix D.

Although the 18-month analysis is intended to represent impacts on the full 18-month study sample of 17,026 persons, the impact estimates reported were obtained from follow-up data for a subsample of 14,941 persons (88 percent of the 18-month study sample) because the required follow-up data were available only for this subsample (see Appendix D).[13] Hence, the impact findings for each of the subgroups discussed above, and for all other subgroups discussed in this report, are based on follow-up data for most but not all of their counterparts in the 18-month study sample.

IMPACTS PER JTPA ASSIGNEE

Because the random assignment process outlined in Exhibit 2.2 produced treatment groups and control groups with no systematic differences at random assignment, the subsequent labor market experience of the control group provides a valid estimate of what the experience of the treatment group would have been if JTPA had not been available to its members. For example, if the mean earnings of the control group were $7,000 during the first year after random assignment, one could infer that the treatment group would have earned this amount (plus or minus a margin to reflect random sampling error) in that same year without assistance from JTPA.

Moreover, if the *actual* mean earnings of the treatment group were $7,500 during that year, one could infer that JTPA increased treatment group earnings by $7,500 minus $7,000, or $500, on average (plus or minus a margin to reflect random sampling error).

Similar logic can be used to estimate program impacts on dichotomous outcomes that are naturally expressed in percentage terms. For example, if 80 percent of the treatment group were employed at some time during the first year after random assignment, and 70 percent of the control group were employed during that time, the best estimate would be that JTPA increased employment by 10 percentage points.

These impact estimates rely exclusively on *direct* comparisons of outcomes for all treatment group members (whether they were subsequently enrolled in JTPA or not) and all control group members. Hence, they represent the average impact of the program on all sample members who were randomly assigned to the group having access to the program: the treatment group. We refer to these findings as estimates of *impacts per assignee*, and they represent the effect of providing treatment group members with *access* to JTPA Title II-A services relative to what they could have accomplished without access to those services.

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[13] At least 18 months of usable follow-up data were obtained from the First Follow-up Survey for 14,446 of these sample members. In addition, to adjust for survey nonresponse bias in the impact estimates for adult women (the only target group for which this was judged to be a problem), we imputed follow-up data from earnings data from state unemployment insurance agencies for as many survey nonrespondents as possible (495). Thus, for the impact analysis we used follow-up data for 14,941 persons, including 98.0 percent of the adult women in the 18-month study sample, 78.5 percent of the adult men, 86.8 percent of the female youths, and 81.5 percent of the male youths.

This comparison of treatment and control group outcomes can be conducted separately for many different subgroups within the 18-month study sample, thereby providing separate program impact estimates for each subgroup. In effect, the experimental design allows for a separate experimental treatment-control group comparison for any sample subgroup that can be defined in terms of common factors measured before or at random assignment. For example, the design can yield separate experimental estimates for women, men, whites, blacks, Hispanics, welfare recipients, high school dropouts, and so on.

The experimental analysis for the study follows standard statistical practice and uses multiple regression to increase the statistical precision of the program impact estimates. Ordinary least squares regression is used for those impact estimates based on continuous outcome variables, such as earnings; and maximum likelihood logistic regression is used for those impact estimates based on dichotomous outcomes that are naturally expressed in percentage terms, such as employment rates. These multivariate techniques control for chance differences between the treatment group and control group in a wide range of baseline characteristics, which are included in the regression model as covariates. Appendix D describes the procedures employed in each case.

But not all treatment group members ultimately became enrolled in JTPA. The estimated impacts per JTPA assignee therefore do not measure the effect of actually participating in JTPA. Instead, they measure the average effect of making the program accessible to eligible applicants, and thus they represent the actual impact that local programs can have by making JTPA Title II-A services available to economically disadvantaged members of the community.[14]

IMPACTS PER JTPA ENROLLEE

As just noted, the estimated JTPA impact per assignee represents the average effect of the program on all treatment group members, whether they became enrolled in JTPA or not. This result can be expressed as a weighted average of the impact on those who were enrolled plus the impact on those who were not enrolled, where the weights are the proportion who were enrolled and the proportion who were not.

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[14] Note, however, that because control group members could and did obtain employment and training services from non-JTPA providers, the comparison of outcomes for treatment group and control group members represents the *incremental* effect of JTPA services relative to the services that could have been received elsewhere in the area.

If the program had *no* effect on those who did not become enrolled, the impact per assignee understates the impact per enrollee because the impact per assignee is the average of a zero impact per nonenrollee plus the average impact per enrollee. In this case, to *infer* the average *impact per JTPA enrollee,* one need only divide the impact per assignee by the proportion of assignees who were enrolled (see Bloom, 1984a).[15]

For example, if the average impact per assignee were $500, and 60 percent of the treatment group were enrolled in JTPA after random assignment, the estimated impact per JTPA enrollee would be $500/.6, or $833. Thus, estimated impacts per enrollee are proportional to estimated impacts per assignee. In this example, the 60 percent enrollment rate implies an estimated impact per enrollee that is 1/.6 or 1.67 times the estimated impact per assignee.

To the extent that treatment group members who did not become enrolled in JTPA were not affected by the program, one can interpret our estimates of the impact per JTPA enrollee as the average effect of enrolling in a JTPA Title II-A program relative to what the enrollees could have accomplished if they had not enrolled in the program.

Nevertheless, to the extent that nonenrolled treatment group members experienced program impacts similar to those of enrollees, our estimates will systematically overstate the true impacts per enrollee.[16] Indeed, when interpreting the estimated impacts per enrollee, the reader should note that some members of the treatment group who were not enrolled in JTPA did receive limited JTPA services. This occurrence reflects a practice by some SDAs of not enrolling applicants immediately after they are judged eligible for the program.

To investigate the extent to which these treatment group nonenrollees received JTPA services, we conducted a separate analysis based on checks of SDA administrative records for a small subsample of treatment group members and on discussions with SDA staff members about what happened to nonenrollees in this subsample (see Appendix F). The administrative records indicated that about 40 percent of the subsample were not later enrolled in Title II-A. Discussions with SDA staff about those nonenrollees indicated that about half received no JTPA service and half received some service. The specific program services received were usually limited, however, mainly constituting attempts to *arrange* services for applicants by referring them to potential employers for on-the-job training; by providing some job search assistance; or by attempting to arrange classroom training. Thus, with few exceptions, it is unlikely that the services treatment group nonenrollees received appreciably affected their future labor market experience, although we cannot be sure about the effect. (See Appendix F and, for more detail, Doolittle, forthcoming.)

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[15] An additional adjustment was made for the fact that 3 percent of the control group enrolled in JTPA, despite the experiment's embargo on their participation. This adjustment, however, had almost no effect on the resulting estimates. For simplicity, then, we ignore the adjustment in the following discussion.

[16] In the unlikely event that JTPA enrollees and nonenrollees in the treatment group experienced opposite impacts from the program, our estimates of program impacts per JTPA enrollee would understate the true impacts per enrollee.

We therefore consider our inferred estimates of impacts per JTPA enrollee as likely upper bounds on the magnitude of the true impact of enrolling in the program. And at the opposite extreme we consider our estimates of impacts per JTPA assignee as likely lower bounds on the magnitude of program impacts on enrollees. In the earlier example, then, the lower bound on the true impact on enrollees would be $500 (the impact per assignee) and the upper bound would be $833 ($500 divided by the 0.6 enrollment rate).

THE INCREMENTAL IMPACT OF JTPA

Whether the impacts are reported per JTPA assignee or per JTPA enrollee, they reflect the *incremental* effect of JTPA services beyond what sample members could have accomplished without access to JTPA, that is, *with* access to services from non-JTPA providers. In other words, they reflect the effect of adding JTPA services to the existing landscape of employment and training programs in the community or region.

The effect of those non-JTPA services is reflected in the labor market outcomes of the control group members. Hence, our impact estimates do not reflect what would happen in the absence of any employment and training services, but rather what would happen without JTPA services. The difference between the outcomes of the treatment group and the control group therefore reflects the effect of the increment in services JTPA availed to treatment group members.

To measure this increment in services, we measured the employment and training services received by treatment and control group members from JTPA and from other providers. In chapters 4 through 7 we report the difference in service receipt between the treatment group and the control group to illustrate the size of the increment that produced the program impacts estimated. Our forthcoming report will also include a detailed analysis of the costs of these services and compare the added costs of additional services provided by JTPA to the estimates of program impacts. This will form the basis for a benefit-cost analysis of the program.

IMPACTS ON ATTAINMENT OF A HIGH SCHOOL CREDENTIAL

One key program outcome, especially for youths, is the attainment of a high school diploma or General Educational Development (GED) certificate. Using responses to the First Follow-up Survey, we identified sample members who both participated in a school or training program *and* attained a high school credential at some time during the 18-month follow-up period. By comparing the percentage of treatment group members who received such a service and attained this credential and the corresponding percentage of control group members, we estimated the impact of JTPA Title II-A programs on this outcome.

We refer to this finding as the program *impact on attainment of a training-related high school credential.* Estimates are presented for each target group, both as a percentage of all assignees and as a percentage of assignees who were high school dropouts when they applied to JTPA. Impacts as a percentage of the larger group indicate the extent to which JTPA increased the educational attainment of the target group as a whole; impacts as a percentage of high school dropouts isolate the impact on only those target group members for whom such an effect was possible.

IMPACTS ON EARNINGS, EMPLOYMENT, AND EARNINGS COMPONENTS

Beyond educational attainment, we also present JTPA impacts on sample members' earnings, their employment, and the components of their earnings. Estimates of impacts on earnings serve as the most comprehensive measure of the ability of the program to increase the labor market success of low-income, economically disadvantaged persons. Estimated impacts on employment measure the program's effect on disadvantaged persons' ability to find and hold jobs. And estimated impacts on the components of earnings indicate the extent to which program-induced earnings gains were the result of an increase in the amount of time worked or an increase in the amount paid for time worked. The analysis of earnings components thus helps to explain how earnings gains (or losses) were produced for those groups that experienced a program impact.

For each target group and service strategy subgroup within each target group, we report estimated impacts on *earnings* separately for each quarter after random assignment and in total for the 18-month follow-up period. The earnings impacts are expressed in dollars and as a percentage of the average earnings of the control group.

Impacts on *employment,* again for each quarter and the follow-up period as a whole, are reported in terms of three different but related measures of employment:

- the *percentage* of the group *employed* at any time during the period,
- the average number of *weeks worked* by members of the group during the period, including zeros for persons who did not work, and
- the average number of *hours worked* during the period, including zeros for persons who did not work.

Finally, the average earnings of sample members are expressed as the product of four *components*:

- the percentage of the group employed at some time during the period (termed *workers per assignee)*,
- the average number of weeks worked by persons who were employed (termed *weeks worked per worker)*,
- the average number of *hours worked per week worked*, and
- the average *earnings per hour worked*.

The relative magnitudes of program impacts on these components were estimated as described in Appendix D and are presented in chapters 4 through 6 to describe how earnings impacts were produced. These estimates also reveal the extent to which JTPA increased sample members' ability to find and hold jobs, the mix of part-time and full-time employment they obtained, and the wage rates they were paid.

Data for the Report

The data used to produce the impact estimates in this report come from five main sources:

- a *Background Information Form* completed by sample members (with assistance from local SDA staff members if necessary) when they applied to JTPA;
- *First Follow-up Survey* interviews that asked sample members about their earnings, employment, and receipt of employment and training services;
- *enrollment and tracking data from the 16 SDAs*, which include information on enrollment, service receipt, and termination status;
- *state unemployment insurance records* on the quarterly wages paid to sample members by local employers; and
- the national *Job Training Quarterly Survey* (JTQS) of JTPA participants' backgrounds and program experiences, drawn from the administrative records of a nationally representative sample of SDAs.

The Background Information Form is the primary source of baseline information on sample members. Administered as part of the program application process at each site over the course of the sample intake period, the form obtained data on applicants' demographic characteristics, education and training, employment history, living situation, and public assistance experience, as well as contact information for the follow-up interviews.

Data from the Background Information Form were used in this report for three main purposes: to describe the 18-month study sample; to define the sample subgroups for which separate impact estimates were calculated; and to construct variables to control for treatment-control group differences in the multiple regression models.

The First Follow-up Survey, as noted earlier, was scheduled for the full experimental sample for periods that varied across sample members from 13 to 22 months after random assignment. This report is based on the experiences of those sample members scheduled for at least 18 months of follow-up (the 18-month study sample), who made up over four-fifths of the full experimental sample.[17]

As described in more detail in Appendix C, the First Follow-up Survey was a 30-minute interview that asked sample members about their earnings, employment, and receipt of employment and training services from the time respondents were randomly assigned to the date of their interview. It also asked questions about current family composition and related issues. The survey was conducted by telephone, with in-person interviews for sample members who could not be reached by telephone.

Exhibit 2.5 shows that the response rate for the First Follow-up Survey was 84.8 percent, which is unusually high, especially for low-income persons. Response rates for females approached 90 percent and were higher than those for males. But even the lowest response rate in the exhibit, 75.7 percent for adult men recommended for the other services strategy, was higher than the rates in many previous studies of economically disadvantaged Americans.

First Follow-up Survey data provided the outcome measures used to estimate program impacts on earnings, employment, and the components of earnings. Appendix D examines the extent to which survey nonresponse biased these impact estimates, using earnings data from state unemployment insurance (UI) agencies for both survey respondents and nonrespondents. This analysis indicates that survey nonresponse bias did not affect the estimates of earnings and employment impacts for adult men and out-of-school youths. For adult women, however, survey nonresponse introduced a small bias, which we adjusted for in the impact estimates using UI earnings data for survey nonrespondents (see Appendix D).

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[17] The Second Follow-up Survey has been administered to about a third of the experimental sample.

Exhibit 2.5 Response Rates in the First Follow-up Survey: 18-Month Study Sample, by Target Group and Service Strategy Subgroup

	Percentage responding to the survey				
Service strategy subgroup	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths*[a] *(4)*	*Male youths*[a] *(5)*
Classroom training	86.1%	88.1%	80.6%	88.4%	83.8%
OJT/JSA	86.2	89.3	82.7	90.0	86.2
Other services	81.2	86.4	75.7	85.1	78.7
All subgroups	84.8	88.2	80.3	87.7	82.7

Sources: Unadjusted frequencies based on Background Information Form responses and First Follow-up Survey responses.

a. Out-of-school youths only.

Computerized administrative records from the 16 SDAs in the study provided information on JTPA enrollment rates for sample members, the amount of time JTPA enrollees spent in the program, the specific program services they received, and whether they were employed when they left the program.

These enrollment and tracking data were used to describe the JTPA program services received by sample members, to compare the specific program services received by persons recommended for the three service strategies, and to identify control group members who entered JTPA during their 18[illegible]onth embargo period (3 percent did so).

We used quarterly earnings data from state UI agencies for 14 of the 16 sites in three ways (see Appendix D).[18] First, we used UI data for the five quarters before random assignment to construct variables to control for treatment-control group differences in pre–random assignment earnings in the multiple regression models used to estimate program impacts. Second, as mentioned above, we used UI data for the first four quarters after random assignment to ascertain any survey nonresponse bias in the survey-based estimates of program impacts on earnings. And finally, we used UI data for the first six quarters after random assignment (the full 18-month follow-up period) to adjust for nonresponse bias in the survey-based estimates of impacts on the earnings of adult women (the only target group, as noted above, for which nonresponse bias was large enough to warrant the adjustment).

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[18] We were unable to obtain state UI records for the Marion, Ohio, site. In addition, problems with the UI records we received for the Jersey City, New Jersey site precluded using that information in this report.

Our last data source, the Job Training Quarterly Survey (funded by the U.S. Department of Labor, conducted by the U.S. Bureau of the Census, and reported by Westat, Inc.), collects background information on JTPA enrollees and terminees from a standing sample of 142 SDAs, selected to provide a statistically valid representation of the JTPA Title II-A and Title III programs nationally. Drawn quarterly from on-site file searches at each participating SDA, JTQS data are the primary source of current information about the number, characteristics, and program services of JTPA enrollees and terminees around the country. We used these data to compare our 18-month study sample and the population of adults and out-of-school youths in JTPA Title II-A nationally during the sample intake period for this study. This analysis, reported in Appendix B, compares the two groups on the basis of JTQS data on their baseline characteristics, the duration of their enrollments, the mix of program services they received, and their labor market status upon termination from the program.

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Context: Study Sites, 18-Month Study Sample, and Patterns of JTPA Enrollment and Service Receipt

THIS chapter describes the study sites and sample for this report. The first section catalogs characteristics of the 16 service delivery areas (SDAs)[1] that agreed to participate in the national JTPA study and compares those characteristics—of the local population and economy, the JTPA programs in place, program participants, and program services—with averages for the broader group of 649 SDAs nationally during the years the study sample was selected.

The second section of the chapter presents a more detailed examination of the 17,026 members of the 18-month study sample and the composition of the main subgroups analyzed in this report. Finally, the last section details patterns of JTPA enrollment and service receipt among these subgroups. All of these topics are analyzed in more depth in the companion volume on the study's implementation (Doolittle, forthcoming).

The 16 Study Sites

As noted in Chapter 2, the 16 study sites were recruited from among those SDAs in the continental United States with at least 500 persons ending their enrollment in Title II-A-funded services ("terminees" in the language of program reporting) in program year 1984.[2]

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[1] In JTPA parlance "service delivery area" refers to both the local administrative agency for the program and the geographical area it serves. Most SDAs provide some specific program service themselves, but many also contract with other providers of employment and training services.

[2] Program year 1984 (July 1984 through June 1985) was the most recent year for which data were available at the time site selection for the study began.

Exhibit 3.1 Key Facts about the 16 Study Sites

Site name	*SDA name*	*Census region*	*Largest city*	*Size of the 18-month study sample*
Fort Wayne, Ind.	Northeast Indiana	Midwest	Fort Wayne	2,559
Coosa Valley, Ga.	Coosa Valley, Ga.	South	Rome	1,806
Corpus Christi, Tex.	Corpus Christi/Nueces County, Tex.	South	Corpus Christi	1,498
Jackson, Miss.	Capital Area, Miss.	South	Jackson	1,375
Providence, R.I.	Providence/Cranston, R.I.	Northeast	Providence	1,277
Springfield, Mo.	Job Council of the Ozarks, Mo.	Midwest	Springfield	1,202
Jersey City, N.J.	Jersey City, N.J.	Northeast	Jersey City	1,170
Marion, Ohio	Crawford/Hancock/ Marion/Wyandot Counties, Ohio	Midwest	Marion	1,083
Oakland, Calif.	Oakland, Calif.	West	Oakland	1,043
Omaha, Neb.	Greater Omaha, Neb.	Midwest	Omaha	956
Larimer County, Colo.	Larimer County, Colo.	West	Fort Collins	668
Heartland, Fla.	Heartland, Fla.	South	Lakeland	597
Northwest Minnesota	Northwest Minnesota (Crookston and Thief River Falls)	Midwest	Thief River Falls	498
Butte, Mont.	Concentrated Employment Program, Mont.	West	Butte	477
Decatur, Ill.	Macon/De Witt Counties, Ill.	Midwest	Decatur	471
Cedar Rapids, Iowa	East Central Iowa	Midwest	Cedar Rapids	346
All sites				*17,026*

The map in Chapter 2 (Exhibit 2.1) shows the location of these sites and lists the abbreviated site names used in this report. The formal name, census region, and largest city of each SDA, and the size of each SDA, and the size of the 18-month study sample from each, are shown in Exhibit 3.1. Throughout this chapter, the exhibits list the sites in descending order by the size of the study sample at the site.

These 16 sites are spread throughout the nation, with two in the Northeast, four in the South, seven in the Midwest, and three in the West. They include sites located in large metropolitan areas with large minority populations (Jersey City and Oakland), others located in predominately rural areas or small towns (Coosa Valley, Georgia; Marion, Ohio; Northwest Minnesota; Butte, Montana), and still others with a mixture of urban, suburban, and rural areas (Fort Wayne; Omaha; Decatur, Illinois). The size of the largest city within each of these SDAs ranges from 372,000 in Oakland and 336,000 in Omaha to under 10,000 in Northwest Minnesota's Thief River Falls (1990 U.S. census data).

No large central cities are included among the study sites. JTPA operations in many central cities are decentralized, with service providers playing an important role in intake and assessment. In Los Angeles, for example, at the time of site selection over 50 organizations were involved in client intake for the program. Because the research design involved coordinating the random assignment with client intake and assessment, and in a site like Los Angeles those procedures often varied slightly from office to office, the research team was unable to develop workable study procedures for this type of site. Nevertheless, two smaller SDAs in large metropolitan areas (Jersey City and Oakland) did participate in the study, and they have many of the same characteristics (in terms of clients, economic conditions, and service availability) as large central cities.

Population Characteristics

As shown in Exhibit 3.2, the sites were also quite diverse in population densities and poverty rates.[3] Three entirely urban SDAs stand out in population density (Providence, Jersey City, and Oakland), while Northwest Minnesota and Butte, Montana, fall at the other extreme. Fort Wayne's relatively low population density is an average of the city of Fort Wayne and the surrounding eight predominately rural counties that are also part of this SDA. The average for the 16 sites is above that for the nation as a whole, at least in part because rural SDAs with only a small number of participants were not recruited to participate in the study.

The poverty rates, shown in the right-hand column of the exhibit, show similar variety.[4] The sites containing large metropolitan areas with large minority populations—Jersey City and Oakland—had the highest poverty rates, but other sites with minority populations such as Corpus Christi (largely Hispanic), Jackson, Mississippi (black), and Providence (black and Hispanic) also had higher-than-average rates. Two predominately rural sites (Coosa Valley, Georgia and Northwest Minnesota) and one with a mix of urban and rural (Heartland, Florida) had poverty rates slightly above the 16-site and national averages.

Economic Conditions

Economic conditions at the sites, summarized in Exhibit 3.3, reflect differences in both regional economic conditions and the local economic base. As is the case nationally, the average unemployment rates (column 1) mask larger differences among the sites.[5] Corpus

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[3] Note that in this and the following exhibits, the present site and national averages are unweighted.

[4] The poverty rates reported in the JTPA Annual Status Report file are based on information from the 1980 census, which collected data on annual income in 1979.

[5] The unemployment rates presented are for the labor force living in the geographic area included in each SDA.

Exhibit 3.2 Selected Population Characteristics of the 16 Study Sites

Site	*Residents per square mile,[a] 1989*	*Percentage of residents in poverty, 1979*
Fort Wayne, Ind.	160	5.9%
Coosa Valley, Ga.	110	10.7
Corpus Christi, Tex.	360	13.4
Jackson, Miss.	360	12.8
Providence, R.I.	4,680	12.1
Springfield, Mo.	80	10.1
Jersey City, N.J.	7,000	18.9
Marion, Ohio	120	7.2
Oakland, Calif.	6,620	16.0
Omaha, Neb.	550	6.7
Larimer County, Colo.	70	5.9
Heartland, Fla.	100	11.3
Northwest Minnesota	10	11.1
Butte, Mont.	10	7.5
Decatur, Ill.	150	7.8
Cedar Rapids, Iowa	90	6.0
16-site average	*1,279*	*10.2*
National average, all SDAs	*733*	*9.7*

Source: Unweighted annual averages calculated from JTPA Annual Status Report computer files produced by the U.S. Department of Labor.
a. Of the service delivery area (SDA).

Christi's residents, for example, experienced persistently high unemployment during the late 1980s, as the oil industry suffered an extended slump. At the other extreme, Providence's low unemployment rate was the result of the New England region's high technology boom of the same period, while the low rates in Fort Wayne, Omaha, and Cedar Rapids reflect the economic resurgence some mid-sized metropolitan areas in the Midwest were enjoying at the time. Decatur's high unemployment, however, illustrates that the recovery was not ubiquitous; in this manufacturing and food processing center, the recovery of the mid- to late 1980s was weak.

The variation in the average earnings of the population in each site (column 2) reflects in part the wage disparities between urban areas (for example, Oakland versus Northwest Minnesota) and in part the concentration of high wage industries in some sites (petroleum

Exhibit 3.3 Selected Economic Conditions at the 16 Study Sites

Site	*Mean unemployment rate, 1987-89 (1)*	*Mean earnings, 1987 (2)*	*% employed in manufacturing, mining, or agriculture, 1988 (3)*	*Annual growth in retail and wholesale earnings, 1989 (4)*
Fort Wayne, Ind.	4.7%	$ 18,700	33.3%	-0.1%
Coosa Valley, Ga.	6.5	16,000	42.8	2.1
Corpus Christi, Tex.	10.2	18,700	16.8	-15.5
Jackson, Miss.	6.1	17,600	12.8	-2.4
Providence, R.I.	3.8	17,900	28.0	9.7
Springfield, Mo.	5.5	15,800	19.4	-1.8
Jersey City, N.J.	7.3	21,400	20.9	9.9
Marion, Ohio	7.0	18,600	37.7	1.7
Oakland, Calif.	6.8	23,000	14.6	3.0
Omaha, Neb.	4.3	18,400	11.8	1.8
Larimer County, Colo.	6.5	17,800	21.2	-3.1
Heartland, Fla.	8.5	15,700	23.8	-0.3
Northwest Minnesota	8.0	14,100	23.0	2.4
Butte, Mont.	6.8	16,900	9.6	-5.7
Decatur, Ill.	9.2	21,100	27.1	-1.1
Cedar Rapids, Iowa	3.6	17,900	21.9	-0.5
16-site average	*6.6*	*18,100*	*22.8*	*0.0*
National average, all SDAs	*6.6*	*18,167*	*23.4*	*1.5*

Source: Unweighted annual averages calculated from JTPA Annual Status Report computer files produced by the U.S. Department of Labor.

Note: Missing data for certain measures precluded using the same year across columns.

in Corpus Christi and heavy manufacturing in Fort Wayne and Decatur).[6] These differences in the local economic base are furthur illustrated in column 3 of Exhibit 3.3, which displays the percentage of workers employed in the goods-producing industries of manufacturing, mining, and agriculture.

The last column of Exhibit 3.3, on annual growth in retail and wholesale earnings during 1989, captures the effects of economic conditions in that year on each SDA. Corpus Christi's economic downturn is starkly visible (a negative 15.5 percent), as is the economic boom in the Northeast during the late 1980s (see Jersey City with a growth rate of 9.9 percent and Providence with 9.7 percent). Nevertheless, on all of these measures the 16-site average is quite similar to the national average for all SDAs.

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[6] Average earnings are calculated by dividing the total payroll reported by employers in the SDA to federal and state unemployment insurance agencies by the number of employees in the SDA.

Exhibit 3.4 *Selected Characteristics of Title II-A Terminees at the 16 Study Sites, Program Years 1987-1989*

	Percentage of all terminees			
Site	*Youths, ages 14-21* [a] *(1)*	*White, non-Hispanic (2)*	*Black, non-Hispanic (3)*	*Hispanic (4)*
Fort Wayne, Ind.	46%	74%	22%	3%
Coosa Valley, Ga.	43	80	20	0
Corpus Christi, Tex.	45	21	8	71
Jackson, Miss.	58	14	85	0
Providence, R.I.	45	34	38	21
Springfield, Mo.	39	95	3	1
Jersey City, N.J.	55	5	68	21
Marion, Ohio	41	95	3	2
Oakland, Calif.	44	7	68	6
Omaha, Neb.	37	42	51	4
Larimer County, Colo.	20	78	2	17
Heartland, Fla.	42	57	37	5
Northwest Minnesota	47	95	0	3
Butte, Mont.	39	90	0	3
Decatur, Ill.	44	60	39	0
Cedar Rapids, Iowa	50	87	9	1
16-site average	*44*	*58*	*28*	*10*
National average, all SDAs	*44*	*61*	*26*	*10*

Source: Unweighted annual averages calculated from JTPA Annual Status Report computer files produced by the U.S. Department of Labor.

a. Includes both out-of-school and in-school youths. The 18-month study sample does not include in-school youths or youths under age 16.

PARTICIPANT CHARACTERISTICS

The sites also exhibited diversity in their program participants. Exhibits 3.4 and 3.5 display selected characteristics of program terminees during the sample intake period.[7] The large differences in ethnic distribution across the sites reflect differences both in the local population generally and in the populations eligible for JTPA. In particular, the SDAs with large metropolitan areas have a much higher proportion of black and Hispanic terminees than do SDAs in rural areas. The highest minority percentages are in Corpus Christi (71 percent

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[7] The frequencies shown in Exhibits 3.4 and 3.5 are for all JTPA Title II-A terminees during the program years listed and are based on data in the JTPA Annual Status Reports (JASR) compiled by the Department of Labor. These JASR data are the best source of information on individual SDAs and the people they serve, but they do not allow for separate breakdowns of out-of-school and in-school youths. The latter group was excluded from the National JTPA Study (as explained in Chapter 2). Appendix B presents an alternative analysis comparing JTPA enrollees and terminees nationally, based on data from

Exhibit 3.5 Selected Barriers to Employment Faced by Title II-A Terminees at the 16 Study Sites, Program Years 1987-1989

	Percentage of all terminees [a]						
Site	*Long-term AFDC recipients* [b] *(1)*	*High school dropouts* [c] *(2)*	*Unemployed 15+ weeks in past 26 (3)*	*Limited English (4)*	*Physical or mental disability (5)*	*Reading at <7th grade level (6)*	*Ever arrested (7)*
Fort Wayne, Ind.	2%	25%	33%	0%	15%	17%	7%
Coosa Valley, Ga.	4	42	8	0	14	31	2
Corpus Christi, Tex.	8	41	49	1	4	33	8
Jackson, Miss.	21	25	42	0	11	21	3
Providence, R.I.	7	37	45	12	4	29	7
Springfield, Mo.	3	28	34	1	3	7	4
Jersey City, N.J.	7	27	43	3	4	4	3
Marion, Ohio	9	25	73	0	23	23	7
Oakland, Calif.	25	17	25	17	11	25	5
Omaha, Neb.	16	18	49	0	10	9	4
Larimer County, Colo.	3	21	57	1	14	12	5
Heartland, Fla.	6	30	7	1	13	17	13
Northwest Minnesota	13	12	41	0	17	10	4
Butte, Mont.	1	15	64	0	19	5	10
Decatur, Ill.	16	11	39	0	10	26	10
Cedar Rapids, Iowa	11	18	74	2	23	11	6
16-site average	*10*	*25*	*43*	*3*	*12*	*18*	*6*
National average, all SDAs	*9*	*25*	*40*	*3*	*14*	*21*	*8*

Source: Unweighted annual averages calculated from JTPA Annual Status Report computer files produced by the U.S. Department of Labor.

a. Includes adults and both out-of-school and in-school youths ages 14 to 21. The 18-month study sample does not include in-school youths or youths under age 16.

b. Family receiving AFDC for any 24 or more of the 30 months preceding determination of eligibility for JTPA.

c. No high school diploma or General Educational Development (GED) certificate.

Hispanic and 8 percent black), Jackson (85 percent black), Jersey City (68 percent black and 21 percent Hispanic), and Oakland (68 percent black, 6 percent Hispanic, and 19 percent other minorities, mostly Asian not shown). On average, though, the ethnic composition of the sites practically mirrored that of SDAs nationally.

Exhibit 3.5 shows the proportion of all terminees who were facing one of seven selected barriers to employment. Again, the averages for the 16 study sites are very close to the averages for all SDAs. Furthermore, in both the study sites as a group and all SDAs

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the Job Training Quarterly Survey. The appendix indicates that the members of the 18-month study sample were somewhat less likely to have a high school diploma or GED certificate when they applied to JTPA than were their national counterparts.

nationally, the proportion of terminees facing any one of these barriers was relatively low. The only exception was the barrier of limited recent work experience (column 3), which affected on average 43 percent of terminees at the sites and 40 percent nationally.

The site rankings on these employment barriers varied substantially across the measures. For example, terminees in Coosa Valley, Corpus Christi, and Providence had the highest incidence of educational barriers (columns 2 and 6), while terminees in Marion, Butte, and Cedar Rapids were the most likely to have limited recent work experience and a physical or mental disability (columns 3 and 5).

Exhibit 3.6 Selected Characteristics of JTPA Title II-A Programs at the 16 Study Sites, Program Years 1987-1989

	Mean number of adult and youth terminees[a]	*Mean number of weeks enrolled*		*Mean federal program cost per adult terminee*
Site	*(1)*	*Adults (2)*	*Youths*[a] *(3)*	*(4)*
Fort Wayne, Ind.	1,195	16	31	$ 1,561
Coosa Valley, Ga.	1,063	12	15	2,481
Corpus Christi, Tex.	1,049	34	33	2,570
Jackson, Miss.	1,227	8	15	1,897
Providence, R.I.	503	7	5	2,841
Springfield, Mo.	938	17	17	1,898
Jersey City, N.J.	853	16	14	3,637
Marion, Ohio	714	27	26	2,199
Oakland, Calif.	1,396	16	17	2,539
Omaha, Neb.	1,111	11	12	2,404
Larimer County, Colo.	354	32	26	1,937
Heartland, Fla.	1,793	15	24	1,782
Northwest Minnesota	430	29	28	2,371
Butte, Mont.	576	21	19	2,665
Decatur, Ill.	525	29	25	3,039
Cedar Rapids, Iowa	658	31	23	2,212
16-site average	*899*	*20*	*21*	*2,377*
National average, all SDAs	*1,177*	*20*	*22*	*2,241*

Source: Unweighted annual averages calculated from JTPA Annual Status Report computer files produced by the U.S. Department of Labor.

a. Includes adults and both out-of-school and in-school youths ages 14 to 21. The 18-month study sample does not include in-school youths or youths under age 16.

PROGRAM CHARACTERISTICS

Title II-A operations differed widely across the sites in terms of the size of the program, the average duration of program services, and program costs. As shown in Exhibit 3.6, the programs ranged in size from 354 terminees annually in Larimer County, Colorado to 1,793 in Heartland, Florida, over the two-year period.[8] The range for the average length of time terminees spent in the program was also large: for adults the average number of weeks enrolled ranged from a low 7 in Providence to a high of 34 in Corpus Christi, while the range for youths was from 5 to 33, in the same two cities. Average annual federal costs per adult terminee reflected both the differences in the lengths of enrollment and the higher service costs (based on higher office rental and salary rates) in large metropolitan areas such as Jersey City and Oakland. Note that the sites as a group differed from the national average in program size (number of terminees), again because the study did not include very large SDAs.

SDAs have the responsibility for selecting and defining the roles of other organizations that will provide JTPA-funded services. These providers range from other public agencies to community-based and other nonprofit organizations to proprietary schools and private sector firms. Exhibit 3.7 displays the variety in service providers that contracted with the 16 SDAs to supply employment and training services during the sample intake period.

Public educational institutions—vocational-technical schools, community colleges, and universities—provided classroom training in 14 sites, and proprietary schools were providers in half of the 16 sites. Arranging for subsidized on-the-job training (OJT) positions in the private sector was done by SDA staff members themselves (8 SDAs) or with the assistance of the state job service, which played a role in 2 of the SDAs; in a fourth a community-based organization was also involved. The job service alone arranged for OJT in another 2 SDAs, and a community college arranged for the service in 1 SDA. In 2 SDAs private sector firms arranged for some OJT positions. A wide variety of organizations provided JTPA-funded job search assistance, although the SDA, the job service, or both were the most common.[9] Basic education was provided by public schools or community colleges in 9 of the 12 sites offering it. The remaining 4 SDAs did not offer basic education as a discrete, identifiable service.

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[8] The average number of terminees annually during the period of the study's random assignment is not related to the sample size in each SDA in any simple way, because the duration of the random assignment varied across sites.

[9] Two SDAs (Providence and Cedar Rapids) did not offer job search assistance as a discrete service, instead offering it only as an integrated part of other services.

Exhibit 3.7 Most Common Service Providers Used by JTPA Title II-A Programs at the 16 Study Sites, by Specific Program Service

	Specific program service			
Site	*Classroom training in occupational skills (1)*	*On-the-job training (2)*	*Job search assistance (3)*	*Basic education (4)*
Fort Wayne, Ind.	Proprietary school Vocational-technical school	SDA	Community-based organization SDA	Not provided
Coosa Valley, Ga.	Community college Vocational-technical school	Community-based organization Private sector firm	Community-based organization	Community college
Corpus Christi, Tex.	Community-based organization Community college Proprietary school	Private sector firm State job service	Community-based organization	Community college
Jackson, Miss.	Community-based organization Community college Proprietary school	State job service SDA	Community-based organization State university	Public school
Providence, R.I.	Community-based organization Proprietary school	SDA	Not provided	Not provided
Springfield, Mo.	Vocational-technical school	SDA	SDA	Public school Vocational-technical school
Jersey City, N.J.	Community-based organization Proprietary school Vocational-technical school	SDA	Community-based organization SDA	Proprietary school
Marion, Ohio	Community college Vocational-technical school	SDA	SDA	SDA
Oakland, Calif.	Community-based organization Proprietary school SDA	Community-based organization SDA	Community-based organization Proprietary school SDA	Community-based organization
Omaha, Neb.	Community-based organization Community college Proprietary school	SDA	Community-based organization SDA	Not provided

(Continued)

Exhibit 3.7 Most Common Service Providers Used by JTPA Title II-A Programs at the 16 Study Sites, by Specific Program Service (continued)

	Specific program service			
Site	*Classroom training in occupational skills (1)*	*On-the-job training (2)*	*Job search assistance (3)*	*Basic education (4)*
Larimer County, Colo.	Vocational technical school/ community college[a]	SDA	State job service SDA	Public school Vocational technical school/ community college
Heartland, Fla.	Community college Public School Proprietary school Vocational-technical school	SDA	State job service	Community college Public school
Northwest Minnesota	Community college State university Vocational-technical school	State job service	State job service	Not provided
Butte, Mont.	Community-based organization Community college Public school	State job service	State job service	Public school
Decatur, Ill.	Community college	Community college	Community college	Public school
Cedar Rapids, Iowa	Community college Vocational-technical school	State job service SDA	Not provided	Community college

Source: Information collected by Manpower Demonstration Research Corporation (Part A contractor of the National JTPA Study), during SDA visits.

Note: Information on the last two categories of program services—work experience and miscellaneous services—examined in this report is not shown because the former was rarely offered and the latter were too numerous to represent here.

a. In Larimer County the vocational-technical school became a community college during the course of the study.

A further important choice facing SDAs was whether to provide classroom training in occupational skills by referring clients to training providers; by "purchasing" a class for several clients at once through a contract with a service provider; or by pursuing both approaches. On the one hand, if an adequate service provider network existed, individual referrals allowed SDA staff to match the training to the interests and needs of specific clients (rather than recruit to fill a class); referrals also avoided the possibility of "flooding" specialized labor markets with numerous program completers at once. On the other hand, purchasing a class allowed the SDA to exert more control over course offerings and curriculum and—if staff members were able to identify occupations in demand—to pinpoint JTPA training resources where there would be a strong demand for graduates.

The 16 SDAs in the study showed considerable diversity in how they made this strategic choice:

- *Six SDAs relied exclusively on individual referrals to training providers.* These included three rural sites (Marion, Northwest Minnesota, and Butte) and three mixed urban-rural sites (Fort Wayne, Decatur, and Cedar Rapids), which relied primarily on public vocational-technical institutes or other public technical or community colleges. These sites tended to emphasize classroom training in occupational skills less than other sites. In virtually all target groups at each of these sites, the percentage of JTPA enrollees who received classroom training in occupational skills was less than the 16-site average for all JTPA enrollees in that target group. (The sole exception was adult men in Butte, Montana.)[10]

- *Four SDAs relied exclusively on contracts to purchase classes.* The four (Jackson, Providence, Omaha, and Heartland) were in urban or mixed urban-rural areas and wrote from five to nine contracts each within a program year. The training was for occupations including truck driving, security guard, retail sales, automotive maintenance, food preparation, marketing, clerical, photocopy machine repair, and home health aide. Two SDAs with a much higher-than-average enrollment in classroom training in occupational skills (Omaha for all four target groups and Jackson for adults) fell into this category.

- *The remaining six SDAs used a mixture of individual referrals and class contracts.* Three of these SDAs, which were in larger, urban areas (Corpus Christi, Jersey City, and Oakland) relied on community-based organizations for training contracts in addition to public vocational-technical institutes, colleges, or proprietary schools. The other three, which included medium-sized towns and rural areas (Coosa Valley, Springfield, and Larimer County) relied primarily on public vocational-technical schools and colleges. This category included some SDAs enrolling a larger-than-average percentage of their sample in classroom training in occupational skills: Jersey City for all target groups, Oakland for adults, and Corpus Christi for youths.[11]

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[10] Unfortunately, the enrollment and tracking data collected from most sites did not include information on the occupation for which people were trained, and the multiplicity of individual referrals prevented the research team from examining contracts for each training placement that would identify the occupation.

[11] As for the first group, data limitations precluded a complete examination of the occupations involved. But the available information on class contracts at these sites suggests the training was for occupations similar to those noted for the second group.

AGENCY STANDARDS AND PROGRAM PERFORMANCE

As noted in Chapter 1, the Department of Labor has set certain standards for the performance of JTPA service delivery areas. Exhibit 3.8 lists the employment standard set for adults in each of the 16 SDAs studied (termed "predicted" performance) and their actual performance on that standard in program year 1988; Exhibit 3.9 does the same for two standards for youths: the "positive termination rate" and the "entered employment rate."[12] The columns labeled "difference" in the two exhibits indicate the difference between expected and actual performance on these three indicators.[13]

It is quite obvious that on all three measures the study sites include some that performed much better than the standard set for them, others that slightly exceeded theirs, still others that failed to meet theirs. On average, though, the 16 sites exceeded their predicted rate by an amount equal to or only slightly smaller than the amount SDAs nationally did.

SERVICES RECEIVED AT THE STUDY SITES

Although the study sites are similar to SDAs nationally in many ways, they exhibit one important difference from their counterparts nationally: they emphasized classroom training and job search assistance more, and on-the-job training and miscellaneous services less. Appendix B presents detailed comparisons of the services received by JTPA enrollees in the 18-month study sample and those received by JTPA terminees nationally.[14] The pattern of more classroom training and job search assistance in the study sites than was the case nationally, and less OJT and miscellaneous services, was apparent for all four target groups.

The analysis of program impacts presented in this report partly controls for these differences between services received by the study sample and those received nationally by JTPA participants, by presenting separate impact findings for sample subgroups who were

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[12] The predicted performance levels are set by DOL regression models that control for the characteristics of both the SDA's labor market and its Title II-A terminees. In most, but not all, of the sites these adjusted standards were the level against which states assessed local performance for the purpose of allocating incentive grants. In some cases the state agency would make further adjustments to the standard produced by the regression model, to reflect special circumstances not taken into account by that model.

[13] The State of Georgia chose not to use the youth positive termination rate as a standard in program year 1988, and so the standard is not reported for Coosa Valley. Similarly, Jersey City, Omaha, and Larimer County were in states not using the youth entered employment rate. In calculating the 16-site and national averages in these exhibits, we excluded any sites not using the standard in question.

[14] Appendix B compares enrollment and tracking data from 16 SDAs on the services received by treatment group members who were enrolled in JTPA during the follow-up period and Job Training Quarterly Survey data on the services received by JTPA terminees nationally who were enrolled in the program during the sample intake period for this study.

Exhibit 3.8 *Agency Performance Standards and JTPA Title II-A Performance at the 16 Study Sites: Entered Employment Rates of Adult Terminees, Program Year 1988*

	Entered employment rate		
Site	*Actual (1)*	*Predicted (2)*	*Difference, in % points (3)*
Fort Wayne, Ind.	84.0%	72.4%	11.6%
Coosa Valley, Ga.	83.5	68.2	15.3
Corpus Christi, Tex.	72.0	67.1	4.9
Jackson, Miss.	67.6	69.2	-1.6
Providence, R.I.	74.3	70.2	4.1
Springfield, Mo.	89.0	76.4	12.6
Jersey City, N.J.	86.5	64.2	22.3
Marion, Ohio	55.5	59.4	-3.9
Oakland, Calif.	67.4	66.1	1.3
Omaha, Neb.	65.0	65.7	-0.7
Larimer County, Colo.	68.0	69.5	-1.5
Heartland, Fla.	74.5	68.7	5.8
Northwest Minnesota	73.5	69.1	4.4
Butte, Mont.	74.0	67.1	6.9
Decatur, Ill.	79.4	65.1	14.3
Cedar Rapids, Iowa	76.9	73.1	3.8
16-site average	*74.5*	*68.2*	*6.2*
National average, all SDAs	*74.2*	*67.3*	*6.9*

Source: Unweighted annual averages calculated from JTPA Annual Status Report (JASR) computer files produced by the U.S. Department of Labor.

Notes: The "entered employment rate" is the percentage of all adult terminees who had found a job before terminating their enrollment in JTPA. The "predicted" entered employment rate is based on the JTPA performance standard reported in JASR, program year 1988.

recommended for different service strategies and consequently received different clusters of JTPA services.

A Profile of the 18-Month Study Sample

The 18-month study sample comprises all experimental sample members whose First Follow-up Survey interview was scheduled for 18 or more months after their random assignment.[15] Two-thirds of the 17,026 members of this sample were in the treatment group and one-third were in the control group.

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15. The only exceptions were treatment group members excluded to maintain a constant 2/1 treatment-control group ratio.

Exhibit 3.9 Agency Performance Standards and JTPA Title II-A Performance at the 16 Study Sites: Positive Termination Rates and Entered Employment Rates of Youth Terminees, Program Year 1988

	Positive termination rate[a]			*Entered employment rate*[a]		
Site	*Actual (1)*	*Predicted (2)*	*Difference, in % points (3)*	*Actual (4)*	*Predicted (5)*	*Difference, in % points (6)*
Fort Wayne, Ind.	77%	75%	2%	50%	38%	12%
Coosa Valley, Ga.	n/a	n/a	n/a	48	41	7
Corpus Christi, Tex.	78	72	6	48	48	0
Jackson, Miss.	76	72	4	34	44	-10
Providence, R.I.	75	78	-3	54	46	8
Springfield, Mo.	94	76	18	70	56	14
Jersey City, N.J.	85	80	5	n/a	n/a	n/a
Marion, Ohio	74	75	-1	44	38	6
Oakland, Calif.	73	78	-5	50	45	5
Omaha, Neb.	81	73	8	n/a	n/a	n/a
Larimer County, Colo.	72	74	-2	n/a	n/a	n/a
Heartland, Fla.	77	74	3	49	35	14
Northwest Minnesota	76	78	-2	38	44	-6
Butte, Mont.	86	76	10	56	45	11
Decatur, Ill.	74	74	0	25	10	15
Cedar Rapids, Iowa	66	78	-12	60	50	10
16-site average	*78*	*76*	*2*	*48*	*41*	*7*
National average, all SDAs	*81*	*75*	*6*	*50*	*41*	*9*

Source: Unweighted averages calculated from JTPA Annual Status Report (JASR) computer files produced by the U.S. Department of Labor.

Notes: The "positive termination rate" is the percentage of all youth terminees who, before terminating their JTPA enrollment, had found a job, attained recognized employment competencies established by the Private Industry Council (PIC), completed elementary, secondary, or post-secondary school, enrolled in another training program or an apprenticeship, enlisted in the Armed Forces, or returned to school full-time. The "entered employment rate" is the percentage who had found a job. The "predicted" rate of each of these is based on the JTPA performance standard reported in JASR, program year 1988.

a. Includes both out-of-school and in-school youths ages 14-21. The 18-month study sample does not include in-school youths or youths under age 16.

The sites' contributions to the sample ranged from 2,559 in Fort Wayne to 346 in Cedar Rapids, as shown in the first column of Exhibit 3.10. Target group composition varied by site (columns 2 through 5), because of differences in both the eligible populations and the recruiting and service emphases across sites, as well as certain exclusions from the study. Most notably, youths in Oakland were excluded from the study at the request of the site. The size of the youth target groups at the other study sites also reflects differing emphases on serving in-school versus out-of-school youths. Because in-school youths were not included in the study, those sites that targeted much of their youth program on this group would have a lower percentage of youths in their study sample.

Exhibit 3.11 shows sample sizes in the four study target groups—adult women and men, and female and male out-of-school youths—and three service strategy subgroups: classroom training, OJT/JSA, and other services. Adult women formed about 39 percent of the sample

Exhibit 3.10 Sample Sizes in the 18-Month Study: Full Sample and Target Groups, by Study Site

Site	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths[a] (4)*	*Male youths[a] (5)*
Fort Wayne, Ind.	2,559	937	979	312	331
Coosa Valley, Ga.	1,806	788	407	410	201
Corpus Christi, Tex.	1,498	524	412	335	227
Jackson, Miss.	1,375	410	398	321	246
Providence, R.I.	1,277	376	388	229	284
Springfield, Mo.	1,202	401	427	191	183
Jersey City, N.J.	1,170	471	298	228	173
Marion, Ohio	1,083	421	485	90	87
Oakland, Calif.	1,043	562	481	0	0
Omaha, Neb.	956	512	220	150	74
Larimer County, Colo.	668	318	234	70	46
Heartland, Fla.	597	234	202	93	68
Northwest Minnesota	498	163	224	55	56
Butte, Mont.	477	183	138	78	78
Decatur, Ill.	471	177	219	30	45
Cedar Rapids, Iowa	346	130	114	57	45
All sites	17,026	6,607	5,626	2,649	2,144

Source: Unadjusted frequencies based on Background Information Form responses.
a. Out-of-school youths only.

(6,607 out of 17,026); adult men, a third of the sample; and out-of-school youths about 28 percent (with 15.6 percent female and 12.6 percent male). Within the full sample SDA staff recommended about 36 percent for the classroom training strategy, 38 percent for the OJT/JSA strategy, and 26 percent for the other services strategy.

ASSIGNEES, ENROLLEES, AND CONTROL GROUP MEMBERS

As explained in Chapter 2, this report presents two sets of impact estimates: impacts per JTPA assignee (treatment group member) and inferred impacts per JTPA enrollee (treatment group member who became enrolled in JTPA after random assignment). Exhibit 3.12 shows 63.8 percent of the treatment group became enrolled in JTPA at some point during the 18-month follow-up period. Enrollment rates across the four target groups varied by only a few percentage points, but differed more substantially among the service strategy subgroups. In every target group the highest enrollment was among those recommended for the classroom training strategy, followed by the other services and OJT/JSA strategies, in that order.

Exhibit 3.11 Sample Sizes in the 18-Month Study: Full Sample and Target Groups, by Service Strategy Subgroup

Service strategy subgroup	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths[a] (4)*	*Male youths[a] (5)*
Classroom training	6,113	2,927	1,353	1,193	640
OJT/JSA	6,410	2,322	2,754	612	722
Other services	4,503	1,358	1,519	844	782
All subgroups	17,026	6,607	5,626	2,649	2,144

Source: Unadjusted frequencies based on Background Information Form responses.
a. Out-of-school youths only.

The variation in enrollment across the three service strategy subgroups is not surprising. As discussed in more detail in the accompanying implementation report (Doolittle, forthcoming), the enrollment process was far from automatic. In the case of classroom training, for example, SDA staff had to link program applicants with a training provider that would accept them. For on-the-job training, the staff had to find an employer willing and able to offer the desired training in exchange for a subsidy. Enrollment ultimately occurred when staff entered an applicant's name into the local JTPA management information system, at which point this *enrollee* was counted among program participants for the purposes of meeting JTPA performance standards (see Appendix F for more detail).

Exhibits 3.13 (for adults) and 3.14 (for youths) display selected baseline characteristics of the sample, with separate columns for the control group and JTPA assignees and enrollees. Within each target group there were only very small differences among these three groups.

Exhibit 3.12 Treatment Group Enrollment in JTPA: Full Sample and Target Groups, by Service Strategy Subgroup

Service strategy subgroup	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths[a] (4)*	*Male youths[a] (5)*
Classroom training	72.4%	72.8%	71.2%	71.5%	74.8%
OJT/JSA	56.5	55.4	56.6	57.5	58.5
Other services	62.3	62.4	58.9	63.1	67.7
All subgroups	63.8	64.6	60.8	65.5	66.8
Sample size	*11,474*	*4,465*	*3,759*	*1,814*	*1,436*

Source: Enrollment and tracking data from the 16 service delivery areas (SDAs).
a. Out-of-school youths only.

Exhibit 3.13 Selected Baseline Characteristics of Adults in the Sample: Control Group and JTPA Assignees and Enrollees, by Gender

	Adult women			*Adult men*		
Characteristic	*Controls (1)*	*Assignees (2)*	*Enrollees (3)*	*Controls (4)*	*Assignees (5)*	*Enrollees (6)*
Ethnicity						
White, non-Hispanic	53.6%	54.5%	56.2%	57.9%	57.0%	57.6%
Black, non-Hispanic	31.1	30.8	27.9	29.2	28.8	27.6
Hispanic	12.3	11.4	12.2	9.1	9.7	10.3
American Indian or Alaskan Native	1.1	1.0	1.0	1.3	1.6	1.2
Asian or Pacific Islander	1.8	2.3	2.6	2.6	2.9	3.3
Barriers to employment						
Receiving cash welfare[a]	39.6%	37.9%	37.9%	14.6%	14.6%	14.2%
No high school diploma or GED certificate	30.2	28.2	26.2	31.5	31.2	29.7
Worked less than 13 weeks in past 12 months	53.3	53.8	52.6	40.6	41.5	40.9
Number of barriers						
None of the above	27.6	28.6	30.1	39.7	37.7	39.1
One of the above	34.2	34.5	34.4	39.2	41.1	40.6
Two of the above	28.4	27.8	27.0	16.9	17.9	17.2
All three of the above	9.8	9.2	8.4	4.2	3.4	3.2
Work and training histories						
Ever employed	85.2%	85.6%	86.2%	91.6%	91.3%	91.2%
Mean individual earnings in past 12 months	$2,352	$2,362	$2,386	$4,093	$3,948	$4,011
Hourly earnings in most recent job						
Never employed	14.8%	14.4%	13.8%	8.4%	8.7%	8.8%
Less than $4	34.2	34.3	35.1	18.2	19.8	20.3
$4 or more	51.0	51.3	51.1	73.4	71.5	70.9
Employed upon application	14.6	14.6	15.0	13.8	13.1	12.7
Previously received occupational training	45.2	46.1	46.7	43.7	47.2	47.6
Public assistance status						
Receiving any public assistance[b]	61.9%	58.7%	58.0%	37.8%	37.0%	36.1%
Receiving AFDC	34.8	33.8	34.6	5.8	6.0	6.6
Receiving food stamps	53.3	50.3	49.6	28.7	28.7	28.5
Receiving other public assistance[c]	20.3	18.4	18.2	19.9	18.9	17.9

(Continued)

Exhibit 3.13 Selected Baseline Characteristics of Adults in the Sample: Control Group and JTPA Assignees and Enrollees, by Gender (continued)

	Adult women			*Adult men*		
Characteristic	*Controls (1)*	*Assignees (2)*	*Enrollees (3)*	*Controls (4)*	*Assignees (5)*	*Enrollees (6)*
AFDC history						
Never AFDC case head	47.1%	49.1%	48.7%	91.1%	91.5%	90.7%
AFDC case head less than 2 years	23.0	23.1	24.1	7.3	6.9	7.6
AFDC case head 2 years or more	30.0	27.9	27.2	1.6	1.7	1.7
JTPA required for welfare, food stamps, or WIN program[d]	12.9%	12.3%	11.4%	7.8%	9.6%	8.2%
Household composition						
No spouse or own child present	21.3%	22.5%	20.7%	58.2%	55.8%	54.7%
Own child under age 4, no spouse, present	21.6	20.1	20.4	3.7	3.8	3.2
Own child, none under 4, no spouse, present	35.8	34.9	35.8	5.1	4.6	5.0
Spouse present, with or without own child	21.3	22.6	23.2	33.0	35.8	37.0
Family income in past 12 months						
< $3,000	30.5%	30.8%	29.5%	34.2%	35.8%	35.5%
$3,000 - $6,000	35.4	33.7	33.6	26.6	25.1	24.4
$6,001 - $9,000	16.7	16.1	16.6	16.2	16.2	16.3
> $9,000	17.5	19.4	20.3	22.9	23.1	23.8
Living in public housing						
Yes	11.9%	11.8%	11.3%	7.5%	6.3%	5.5%
No	88.1	88.2	88.7	92.5	93.7	94.5
Age at random assignment						
22 - 29	43.4%	44.2%	44.0%	43.2%	44.6%	45.5%
30 - 44	44.6	43.1	44.0	45.1	43.3	42.3
45 - 54	7.9	8.0	7.4	7.9	7.8	7.8
> 54	4.1	4.7	4.6	3.9	4.3	4.5
Mean	*33.1*	*33.3*	*33.2*	*33.1*	*33.1*	*33.0*
Sample size	*2,142*	*4,465*	*2,883*	*1,867*	*3,759*	*2,286*

Source: Unadjusted frequencies based on Background Information Form responses.

a. AFDC, General Assistance, or other welfare except food stamps.

b. "Any public assistance" includes the following sources of assistance: AFDC, food stamps, unemployment insurance, housing assistance, and other cash assistance.

c. "Other public assistance" includes unemployment insurance, housing assistance, and other (non-AFDC) cash assistance.

d. WIN is the federal Work Incentive program.

Exhibit 3.14 Selected Baseline Characteristics of Out-of-School Youths in the Sample: Control Group and JTPA Assignees and Enrollees, by Gender

	Female youths			Male youths		
Characteristic	*Controls (1)*	*Assignees (2)*	*Enrollees (3)*	*Controls (4)*	*Assignees (5)*	*Enrollees (6)*
Ethnicity						
White, non-Hispanic	47.4%	50.1%	51.2%	52.4%	53.7%	55.2%
Black, non-Hispanic	34.7	32.5	28.2	31.5	29.5	26.5
Hispanic	16.4	15.8	18.8	14.6	14.7	15.8
American Indian or Alaskan Native	0.7	0.8	0.9	0.6	1.1	1.2
Asian or Pacific Islander	0.7	0.8	0.9	1.0	1.0	1.2
Barriers to employment						
Receiving cash welfare[a]	30.8%	30.3%	30.1%	9.6%	11.0%	11.4%
No high school diploma or GED certificate	49.0	49.2	52.5	57.6	59.1	61.7
Worked less than 13 weeks in past 12 months	59.4	60.5	59.9	49.5	47.0	48.7
Number of barriers						
None of the above	21.8	20.8	21.3	22.3	23.9	21.8
One of the above	32.2	32.3	30.0	41.4	40.9	40.4
Two of the above	31.3	32.3	32.9	32.3	29.5	32.3
All three of the above	14.7	14.6	15.9	4.0	5.7	5.5
Work and training histories						
Ever employed	77.3%	76.9%	76.8%	86.0%	83.9%	83.7%
Mean individual earnings in past 12 months	$1,384	$1,305	$1,341	$2,114	$2,071	$2,007
Hourly earnings in most recent job						
Never employed	22.8%	23.1%	23.2%	14.0%	16.1%	16.3%
Less than $4	47.0	47.6	49.0	36.5	34.9	35.5
$4 or more	30.2	29.3	27.9	49.5	49.0	48.2
Employed upon application	14.3	14.4	15.6	11.9	11.4	12.1
Previously received occupational training	25.7	25.5	23.8	31.1	29.7	29.6
Public assistance status						
Receiving any public assistance[b]	48.4%	47.1%	46.3%	26.6%	29.5%	30.8%
Receiving AFDC	27.5	26.6	26.6	4.4	6.2	5.7
Receiving food stamps	40.0	39.3	39.6	22.2	25.0	26.0
Receiving other public assistance[c]	10.1	10.5	9.6	10.2	11.0	11.8

(Continued)

Exhibit 3.14 Selected Baseline Characteristics of Out-of-School Youths in the Sample: Control Group and JTPA Assignees and Enrollees, by Gender (continued)

	Female youths			*Male youths*		
Characteristic	*Controls (1)*	*Assignees (2)*	*Enrollees (3)*	*Controls (4)*	*Assignees (5)*	*Enrollees (6)*
AFDC history						
Never AFDC case head	73.5%	71.0%	70.8%	98.3%	97.8%	97.5%
AFDC case head less than 2 years	20.3	21.9	22.2	1.7	1.9	2.2
AFDC case head 2 years or more	6.2	7.2	6.9	0.0	0.2	0.2
JTPA required for welfare, food stamps, or WIN program[d]	8.1%	7.3%	7.1%	5.5%	5.9%	5.7%
Household composition						
No spouse or own child present	51.8%	52.5%	51.0%	85.7%	85.2%	84.1%
Own child under age 4, no spouse, present	34.5	34.7	35.2	4.0	3.9	3.8
Own child, none under 4, no spouse, present	1.3	1.5	1.6	0.2	0.4	0.4
Spouse present, with or without own child	12.4	11.4	12.2	10.1	10.5	11.8
Family income in past 12 months						
< $3,000	45.5%	45.8%	46.5%	40.8%	39.7%	40.4%
$3,000 - $6,000	27.5	28.5	27.7	27.2	26.3	25.7
$6,001 - $9,000	11.3	9.8	10.0	12.1	11.3	11.8
> $9,000	15.7	15.9	15.8	19.9	22.8	22.1
Living in public housing						
Yes	15.1%	13.9%	13.8%	12.3%	10.7%	10.7%
No	84.9	86.1	86.2	87.7	89.3	89.3
Age at random assignment						
16-19	63.2%	59.4%	59.3%	59.2%	62.1%	63.9%
20-21	36.8	40.6	40.7	40.8	37.9	36.1
Mean	*18.9*	*19.0*	*19.0*	*19.0*	*18.9*	*18.9*
Sample size	*835*	*1,814*	*1,188*	*708*	*1,436*	*959*

Source: Unadjusted frequencies based on Background Information Form responses.

a. AFDC, General Assistance, or other welfare except food stamps.

b. "Any public assistance" includes the following sources of assistance: AFDC, food stamps, unemployment insurance, housing assistance, and other cash assistance.

c. "Other public assistance" includes unemployment insurance, housing assistance, and other (non-AFDC) cash assistance.

d. WIN is the federal Work Incentive program.

The similarity between control group members and JTPA assignees was expected, given the strict random assignment process that created the two groups. Indeed, a detailed statistical comparison of the treatment group (assignees) and the control group, reported in Appendix A, found no systematic or statistically significant differences in baseline characteristics. The similarity between assignees and enrollees is also noteworthy. If the sites encountered special difficulties in arranging services leading to enrollment for a particular subgroup, the assignees in that subgroup would be less likely to enroll in JTPA than other subgroups, and we would then see differences in baseline characteristics between assignees and enrollees. That appears not to have been the case.

TARGET GROUPS

Exhibits 3.13 and 3.14 also serve to distinguish among the four target groups. The adults in the sample tended to be young; around 45 percent were under age 30, and only around 12 percent were age 45 or older upon their application to JTPA. Adult men tended to be the most employable and to have the most extensive work experience. More specifically, nearly 70 percent of the men had a high school diploma or General Educational Development (GED, or high school equivalency) certificate; and men also had the highest average earnings of the four groups and were the most likely to have earned more than $4.00 hourly in their most recent job.

Adult women and female out-of-school youths were considerably more likely than their male counterparts to be receiving public assistance and to have limited recent work experience (less than 13 weeks in the year preceding their application). They were also much more likely to have a dependent child in the household.

The youth target groups were the most disadvantaged. Only about half of all female youths and 40 percent of the male youths had a high school credential. Youths' average earnings in the year before their application were only slightly over half the earnings level of their adult counterparts, and they were much more apt to live in families with less than $3,000 in income over the previous 12 months.[16]

SUBGROUPS FACING SELECTED BARRIERS TO EMPLOYMENT

To determine whether JTPA impacts varied with the degree of labor market disadvantagedness, the 18-month analysis includes separate estimates for subgroups facing selected barriers to employment. Following a framework developed for several studies of

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[16] Differences in the baseline characteristics of the three service strategy subgroups are examined in chapters 4, 5, and 6.

JTPA by the U.S. General Accounting Office (1989, 1990, and 1991), this analysis focuses on the following barriers:

- *welfare receipt,* measured as receiving Aid to Families with Dependent Children (AFDC), General Assistance, or other cash welfare upon application to JTPA;
- *limited education,* measured as a lack of a high school diploma or GED certificate upon application; and
- *limited recent work experience,* measured as having worked less than 13 weeks in the 12 months preceding the application.

The incidence of each of these barriers among control group members, assignees, and enrollees is shown in the second main panel of Exhibits 3.13 and 3.14. Exhibit 3.15 shows the proportion of the treatment group overall and in each target group facing none, one, two, or all three of these barriers to employment. Almost 70 percent of the full sample was facing at least one barrier, but only 6.5 percent was facing all three. There were important differences among the target groups, however. As noted earlier, the out-of-school youths in the sample were the most disadvantaged; here they exhibit the highest incidence of the barriers, with female youths at the extreme with 42.0 percent facing two or three barriers. Adult men were at the other extreme, with 80.8 percent facing none or only one of the barriers.

Exhibit 3.15 Distribution of the Treatment Group among Subgroups Facing a Certain Number of Barriers to Employment: Full Sample and Target Groups

Number of barriers to employment	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths[a] (4)*	*Male youths[a] (5)*
None	31.6%	29.6%	40.3%	22.9%	25.9%
1	38.4	36.6	40.5	35.0	42.4
2	23.5	26.1	16.3	29.6	26.8
3	6.5	7.6	2.9	12.4	5.0
Sample size	*11,474*	*4,465*	*3,759*	*1,814*	*1,436*

Source: Unadjusted frequencies based on Background Information Form responses.

Note: Percentages may not sum to 100 percent because of rounding. The barriers to employment are the receipt of cash welfare (AFDC, General Assistance, or other welfare except food stamps), having no high school diploma or GED certificate, and having worked less than 13 weeks in the 12 months prior to application to JTPA.

a. Out-of-school youths only.

Patterns of JTPA Enrollment and Service Receipt

This section provides a context for understanding the impacts estimated in the later chapters, by describing the service strategies JTPA staff recommended for the sample, the services in which each service strategy subgroup enrolled, and the duration of their enrollment.

SERVICE STRATEGIES RECOMMENDED

After sample members applied to JTPA, a SDA staff member assessed their interests, skills, service needs, and personal preferences for the purposes of recommending an appropriate combination and sequence of program services. The personal preferences of the applicant were often an important factor in recommending a particular cluster of services, since JTPA is a voluntary program and many individuals would not participate in services that did not interest them. For example, some applicants with limited education wanted a job rather than any classroom instruction, and this preference might have lead the intake staff to recommend services such as on-the-job training or job search assistance, even though the staff would otherwise have recommended basic skills training.

As explained in Chapter 2, we categorized sample members by the three clusters of services, or service strategies, that SDA staff advised us reflected the pattern of services they recommended. Exhibit 3.16 shows that the service strategy most often recommended was OJT/JSA, which SDA staff recommended for 37.4 percent of the treatment group overall. This was closely followed by classroom training, recommended for 35.9 percent; other services was the choice for 26.7 percent.

The aggregated figures mask distinct differences in service recommendations by gender and age, as shown in the columns of the exhibit for the target groups.

Exhibit 3.16 Service Strategies Recommended for the Treatment Group: Full Sample and Target Groups

Recommended service strategy	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths[a] (4)*	*Male youths[a] (5)*
Classroom training	35.9%	44.0%	24.6%	44.3%	29.9%
OJT/JSA	37.4	35.0	48.7	23.2	32.9
Other services	26.7	21.0	26.7	32.5	37.3
Sample size	*11,474*	*4,465*	*3,759*	*1,814*	*1,436*

Source: Unadjusted frequencies based on Background Information Form responses.
a. Out-of-school youths only.

- Females, both adults and out-of-school youths, were more likely than males to be recommended for the *classroom training strategy*. Among adults, 44.0 percent of the women, as opposed to only 24.6 percent of the men, were advised to pursue this strategy; among youths, the corresponding figures were 44.3 percent versus 29.9 percent.

- Males, both adults and youths, were more likely than females to be recommended for the *OJT/JSA strategy*. Almost half of all men received this recommendation, as opposed to only just over a third of the women. OJT/JSA was less often recommended for youths, but the gender difference was again substantial (32.9 percent versus 23.2 percent).

- Youths were more apt than adults to be recommended for the *other services strategy*. This strategy was advised for 37.3 percent of male youths and 32.5 percent of female youths but for only 26.7 percent of adult men and 21.0 percent of adult women.

DIFFERENCES IN EMPLOYABILITY ACROSS SERVICE STRATEGY SUBGROUPS

Throughout the later discussions of impacts on the three service strategy subgroups within each target group, it will be important to bear in mind, that the three subgroups differed from one another in important ways. After all, SDA staff made their service strategy recommendations based in large part on each applicant's job skills and experience and needs for more or less intensive employment and training services.

One clear way to summarize the differences among the subgroups is to use estimates of the control group's earnings over the 18-month follow-up period as a measure of *employability*.[17] Throughout this report the earnings of the control group serve as our estimate of what the treatment group would have earned in the absence of the program. Thus, they also indicate the treatment group's employability without access to the program.

Exhibit 3.17 displays the total 18-month earnings of control group members in various target group–service strategy subgroup combinations. Differences in employability across subgroups are readily apparent.

In every target group except adult men, those control group members recommended for the *OJT/JSA* strategy had the highest average earnings over the 18 months following random assignment. Among men the earnings of the other services subgroup were slightly above

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[17] These estimates are based on the First Follow-up Survey data used in the analysis of program impacts in the later chapters.

Exhibit 3.17 *Mean Earnings of the Control Group over the 18-Month Follow-up Period: Full Sample and Target Groups, by Service Strategy Subgroup*

	Mean earnings of the control group			
Service strategy subgroup	*Adult women (1)*	*Adult men (2)*	*Female youths* [a] *(3)*	*Male youths* [a] *(4)*
Classroom training	$ 6,391	$ 11,780	$ 5,936	$ 9,783
OJT/JSA	8,607	12,456	7,620	12,765
Other services	7,960	12,516	5,726	9,839
All subgroups	7,488	12,306	6,225	10,736
Sample size	*2,142*	*1,867*	*835*	*708*

Source: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state unemployment insurance (UI) agencies.
Note: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.
a. Out-of-school youths only.

those of the OJT/JSA subgroup, probably because almost a third of the former group was job ready enough to be recommended for job search assistance only (not shown in the exhibit). The lowest earnings among adults, and hence the least job-ready sample members, were in the classroom training subgroup. In contrast, the least job-ready youths appeared to be concentrated in both the classroom training and the other services subgroup. The most striking finding in the table is the high earnings level of male youth control group members recommended for OJT/JSA—which surpassed even that of adult men in the OJT/JSA subgroup. This estimate suggests that local staff routed a very job-ready group of male youths to OJT/JSA.

DIFFERENCES IN JTPA SERVICE RECEIPT ACROSS TARGET GROUPS AND SERVICE STRATEGY SUBGROUPS

As noted in Chapter 2, the impact of JTPA depends on the difference in services received by those with access to the program and the services they would have received had they been excluded from the program. Here we will simply summarize the JTPA services received by the treatment group overall (including both those who did and those who did not enroll) and by enrollees only.

Exhibit 3.18 Receipt of Specific JTPA Services by the Treatment Group: Full Sample and Target Groups, by Service Strategy Subgroup

	% of treatment group receiving the service				
Specific program service	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths*[a] *(4)*	*Male youths*[a] *(5)*
			Classroom training subgroup		
Never enrolled	27.6%	27.2%	28.8%	28.5%	25.2%
Classroom training in occupational skills	56.2	57.8	55.7	54.8	52.4
Basic education[b]	12.9	10.6	8.8	17.8	23.3
On-the-job training	3.8	3.3	5.4	2.6	4.4
Job search assistance	19.5	17.1	12.4	27.3	30.8
Work experience	4.0	3.9	1.7	5.7	6.5
Miscellaneous[c]	9.9	11.3	9.7	7.7	7.9
			OJT/JSA subgroup		
Never enrolled	43.5%	44.6%	43.4%	42.5%	41.5%
Classroom training in occupational skills	3.3	5.1	2.1	3.3	1.9
Basic education[b]	3.1	2.6	3.6	3.1	2.8
On-the-job training	28.0	28.5	26.6	29.9	30.5
Job search assistance	28.9	26.5	30.2	28.3	32.2
Work experience	2.9	2.6	2.4	5.2	4.2
Miscellaneous[c]	6.5	5.8	6.8	7.1	6.8
			Other services subgroup		
Never enrolled	37.7%	37.6%	41.1%	36.9%	32.3%
Classroom training in occupational skills	9.4	15.6	4.9	9.8	6.5
Basic education[b]	15.7	11.1	6.1	29.7	26.4
On-the-job training	4.7	5.5	4.7	3.9	3.9
Job search assistance	19.7	23.4	24.8	12.2	12.0
Work experience	2.3	2.7	0.9	3.4	3.2
Miscellaneous[c]	31.0	31.5	28.4	28.5	35.3
Sample size	*11,474*	*4,465*	*3,759*	*1,814*	*1,436*

Source: Enrollment and tracking data from the 16 SDAs.

a. Out-of-school youths only.

b. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

Exhibit 3.18 details the specific program services received by *treatment group members* in each service strategy subgroup within the full sample and the target groups. Recall that classroom training in occupational skills was the service defining the classroom training strategy and on-the-job training was the service defining the OJT/JSA strategy. The other services strategy, on the other hand, was defined as recommending neither of those services as the primary one.

The patterns of service receipt were indeed quite different for the three subgroups. In every target group over 50 percent of the classroom training subgroup received the primary service—occupational skills training.

Members of the OJT/JSA subgroup, however, were apt to receive one of two services: on-the-job training or job search assistance. The exhibit also shows that the service strategy definitions were accurate; only a small percentage of the classroom training subgroup received on-the-job training, and only a small percentage of the OJT/JSA subgroup received classroom training in occupational skills.

Exhibit 3.19 focuses in on JTPA *enrollees only* and highlights the two key services received in each target group–service strategy subgroup combination. This breakdown provides further insight into the services received by the first two service strategy subgroups and helps to untangle the more complex patterns of receipt for the other services subgroup in the preceding exhibit. Specifically:

- Most of the treatment group members recommended for the classroom training service strategy received either classroom training in occupational skills, the defining service for that strategy, basic education, a secondary service, or both. Hence, in terms of the services actually received, it is most appropriate to characterize this service strategy as one focused on classroom instruction.

- Most of the treatment group members recommended for the OJT/JSA service strategy received either on-the-job training, the defining service for that strategy, job search assistance, a secondary service, or both. Hence, it is most appropriate to characterize this service strategy as one focused mainly on immediate employment, with or without subsidized training.

- Most of the adult treatment group members recommended for the other services strategy received either job search assistance or miscellaneous services, or both. Most of the youth treatment group members recommended for the other services strategy received either basic education, miscellaneous services, or both. Hence, adults in this subgroup were more likely to receive services that focused on immediate employment, whereas youths were more likely to receive services that focused on basic education and other preemployment services.

Exhibit 3.19 Key JTPA Services Received by Treatment Group Members Who Were Enrolled in the Program: Target Groups, by Service Strategy Subgroup

	% of enrollees receiving one or both services			
Key services in each service strategy subgroup	*Adult women (1)*	*Adult men (2)*	*Female youths[a] (3)*	*Male youths[a] (4)*
		Classroom training subgroup		
Classroom training in occupational skills/ basic education[b]	88.8%	85.5%	86.1%	80.4%
		OJT/JSA subgroup		
On-the-job training/ job search assistance	87.8	86.5	84.8	84.5
		Other services subgroup		
Job search assistance/ miscellaneous[c]	82.3	88.7	–	–
Basic education/[b] miscellaneous[c]	–	–	79.5	83.2
Sample size	*2,883*	*2,286*	*1,188*	*959*

Source: Enrollment and tracking data from the 16 SDAs.

Note: As shown in the bottom panel, the key services received by the other services subgroup differed between adults and out-of-school youths.

a. Out-of-school youths only.

b. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

The findings for classroom training are straightforward, but those for the OJT/JSA and other services strategies require some background on program operations and the research design to be clearly understood.

The OJT/JSA service strategy was intended for applicants seeking employment who, in the judgment of local staff, appeared to need on-the-job training and a wage subsidy to develop the skills necessary to be hired as unsubsidized workers. The initial actions taken to arrange on-the-job training, however, were often very similar to what staff members would do to help an applicant find unsubsidized employment; the first step in both cases was to find an employer interested in hiring a new employee. Furthermore, the applicant might also be seeking an unsubsidized job, often with help from SDA staff in the form of job search assistance. As shown earlier in Exhibit 3.18, across all target groups roughly equal proportions of treatment group members (ranging from 26.5 percent to 32.2 percent) who had been recommended for the OJT/JSA strategy subsequently enrolled in on-the-job training or job search assistance. Thus, in each target group, members of the OJT/JSA subgroup were just as likely to receive JSA as OJT.

The other services strategy, by definition, involved a diverse group of clients. Under the research design, SDA staff recommended this strategy for applicants facing serious employment barriers, who needed basic education or preemployment skills enhancement before they could benefit from classroom training in occupational skills or on-the-job training, or before they could be expected to land a job. The strategy was also deemed appropriate for those needing specialized services, such as a customized combination of classroom and on-the-job training, vocational exploration, job shadowing, and tryout employment, among a large number of other services that varied across sites. These specialized services are grouped together as "miscellaneous services" in the exhibits of this report. Finally, the other services strategy was also appropriate for those who were so obviously employable that they needed only job search assistance. Although job search assistance was a common activity in all three service strategies, it was only in the other services strategy than an applicant could be recommended for this service alone. Thus, within the other services subgroup the difference in the service pattern between adults (who received primarily job search assistance or miscellaneous services) and youths (who received primarily basic education or miscellaneous services), as shown in Exhibit 3.19, suggests that those very job-ready applicants recommended for this strategy were primarily adults, whereas the youths recommended for this strategy tended to have more serious skill deficits that had to be addressed first through basic education or preemployment skills training.

ENROLLMENT PATTERNS OVER TIME AMONG ENROLLEES AND THE TREATMENT GROUP OVERALL

There were also clear differences among the service strategy subgroups and, to a lesser extent, target groups in the duration of their enrollment in Title II-A. As shown in Exhibit 3.20, among treatment group members who were enrolled in JTPA those recommended for the OJT/JSA strategy tended to have the shortest periods of enrollment, whereas those recommended for the classroom training strategy had the longest. The relatively short average enrollments among members of the OJT/JSA subgroup reflect their high rate of receiving job search assistance, a service that tends to be very brief. There were also differences among the target groups, even within service strategies. Adult male enrollees had the shortest period of program participation overall, and in both the classroom training and other services subgroups.

Another way to characterize the services received by the treatment group is to identify a period during which most of its members were enrolled in the program and a period during which most were no longer enrolled. Exhibit 3.21 presents the proportion of each target group–service strategy subgroup combination enrolled in Title II-A in various months after random assignment. There was a sharp drop in enrollment between the third and sixth month, and by the sixth month less than 15 percent of both the OJT/JSA and the other services

Exhibit 3.20 Median Number of Months Enrolled in JTPA among Treatment Group Members Who Were Enrolled: Full Sample and Target Groups, by Service Strategy Subgroup

Service strategy subgroup	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths[a] (4)*	*Male youths[a] (5)*
Classroom training	5.0	5.6	3.8	5.5	4.6
OJT/JSA	2.0	1.9	2.1	2.0	2.2
Other services	2.6	2.4	1.7	3.3	2.9
All subgroups	3.3	3.6	2.5	3.9	3.1
Sample size	*7,316*	*2,883*	*2,286*	*1,188*	*959*

Source: Enrollment and tracking data from the 16 SDAs.
a. Out-of-school youths only.

subgroup was enrolled. The services received by the classroom training subgroup obviously lasted longer. Enrollment rates of the classroom training subgroup dropped to 15 percent or below sometime between the ninth and twelfth month for treatment group members overall, but not until between the fifteenth and eighteenth month for adult women.

JTPA SERVICES RECEIVED BY TREATMENT GROUP NONENROLLEES

The previous subsections focused on the program services received by treatment group members overall and by those who were enrolled in the program. But because the inferred estimates of program impacts per JTPA enrollee in this report require the assumption that treatment group *nonenrollees* experienced no JTPA impact, it is important to review briefly the extent to which nonenrollees did or did not receive JTPA services. This review also provides additional insight into program operations.

To investigate the issue, the implementation research team conducted a separate analysis of JTPA service receipt by a small subsample of those treatment group members who did not become enrolled in the program during the 18-month follow-up period. (See Appendix F and, for more detail, Doolittle, forthcoming.) This analysis found that SDA staff members worked to some extent with approximately half of these treatment group nonenrollees, primarily in attempts to arrange services by, for example, referring them to potential employers for on-the-job training, providing job search assistance, or attempting to arrange classroom training. The remaining half either lost interest in the program or found another training or employment opportunity on their own.

Exhibit 3.21 *Treatment Group Enrollment in JTPA in Selected Months of the 18-Month Follow-up Period: Full Sample and Target Groups, by Service Strategy Subgroup*

	Percentage of the treatment group enrolled in JTPA				
Month	*Full sample (1)*	*Adult women (2)*	*Adult men (3)*	*Female youths*[a] *(4)*	*Male youths*[a] *(5)*
			Classroom training subgroup		
Month 3	56.8%	58.8%	49.1%	61.0%	56.4%
6	35.8	39.6	26.9	38.2	33.1
9	23.7	27.0	16.6	25.5	20.0
12	14.9	17.5	10.5	13.9	14.5
15	13.9	16.0	10.4	12.7	14.2
18	13.1	15.2	9.4	12.6	12.6
			OJT/JSA subgroup		
Month 3	28.3%	26.6%	29.1%	29.9%	29.2%
6	12.5	12.2	12.2	13.5	13.3
9	6.2	6.7	5.4	7.4	6.4
12	3.9	4.5	3.5	4.0	3.6
15	3.9	4.6	3.4	4.0	3.6
18	3.4	4.2	2.5	3.6	3.8
			Other services subgroup		
Month 3	33.9%	30.2%	27.0%	42.7%	43.7%
6	14.0	14.3	11.9	15.8	15.3
9	7.0	9.8	6.1	6.3	4.7
12	4.8	7.2	3.9	4.4	3.0
15	4.6	6.8	3.6	4.1	3.2
18	4.0	6.2	3.3	3.1	2.4
Sample size	*11,474*	*4,465*	*3,759*	*1,814*	*1,436*

Source: Enrollment and tracking data from the 16 SDAs.
a. Out-of-school youths only.

In general, nonenrollees for whom staff tried to arrange OJT or classroom training clearly received much less intensive JTPA services than those treatment group members who actually were enrolled in program services and began to receive training. On the other hand, those referred to job search assistance who never became enrolled may have participated in much the same services as those who did ultimately enroll; but the nonenrollees were generally less successful in finding employment. Overall, about 82 percent of the treatment group (the 64 percent who enrolled plus about half of the 36 percent who did not enroll) had some contact with JTPA, of widely varying degrees of intensity, after their random assignment.

Two final points about these services to nonenrollees should be made. First, it is likely that the JTPA performance standards system is a major reason why local JTPA staff wait until applicants actually begin receiving a formal classroom training or an on-the-job service before enrolling them in the program. As explained earlier in this chapter, the Department of Labor assesses the performance of each SDA based on the success of its program terminees, as measured based on their employment rates, wage rates, and—for youths—a broader measure called "positive termination." But only those individuals who formally enroll in JTPA are counted in this performance standards system, and so the SDAs have an incentive to wait until an applicant actually begins receiving a service designed to increase employability before enrolling him or her. In many SDAs initial assessment, counseling, development of a employability plan, and referrals to potential service providers all typically happen before applicants are formally enrolled in JTPA.

Second, this pattern of services provided to nonenrollees does not bias the estimates of program impacts per *assignee* reported in Chapters 4 through 7, since these measure the impact of access to JTPA on *all* members of the treatment group, whether they become enrolled or not. Furthermore, the benefit-cost analysis that will appear in our forthcoming final report will include estimates of the JTPA costs of serving all those in the treatment group; and thus the comparison of impacts per assignee with costs per assignee necessary for that analysis will also be valid. The finding that some JTPA services were provided to treatment group nonenrollees is cause for caution, however, in interpreting the alternative impact estimates—impacts per enrollee—presented in the following chapters. As explained in Chapter 2, impacts per assignee should be interpreted as a lower bound, and impacts per enrollee, as an upper bound, on the true impact of JTPA on those treatment group members who actually received some program service.

Summary

The most basic conclusion of this chapter is that the study sites and the 17,026 members of the 18-month study sample resemble SDAs and their participants nationally and also include much of their diversity. The sites, though not chosen randomly, include several with very strong economies during the late 1980s, others experiencing modest growth, and still others slowly recovering from job losses in the recession of the early 1980s. Furthermore, the sites' performance on key Department of Labor standards for Title II-A programs showed similar diversity, with both strong and weak performers, as defined by those standards included in the study. As with the sites, the members of the study sample are themselves a diverse group, allowing an analysis of program impacts stemming from each of the three JTPA service strategies as well as impacts on numerous subgroups of interest to policymakers and program planners.

The diversity among the study sites in labor market conditions, in the characteristics of the people they served at the time, and in service duration and costs has a second clear implication for the analysis: We can expect to see wide variation in program impacts across the sites, but the very fact of that diversity will make it difficult to explain the differences with precision.

In addition, the study's definition of three main service strategies recommended by SDA staff did produce distinct subgroups that differed systematically not only in the types of JTPA services they ultimately received but also in their baseline characteristics. As a result, the estimates of program impacts presented in later chapters will offer an assessment of how well these clusters of services—classroom training, on-the-job training/job search assistance (OJT/JSA), and other services—were working for the particular groups of applicants they were designed to serve.

This chapter has also provided a context for understanding the JTPA services both recommended for and received by treatment group members—the source of the program impact. Just under two-thirds (63.8 percent) of the treatment group was enrolled in JTPA at some point in the 18-month follow-up period for this report. In general, the services enrollees received were of a relatively short duration, reflecting JTPA's national emphasis during the 1980s on job placement and low service costs. Those treatment group members recommended for the classroom training strategy were the most likely of the three service strategy subgroups to be enrolled in JTPA, to stay enrolled for a relatively long time, and to receive some form of classroom instruction. Those recommended for the OJT/JSA strategy had the lowest overall enrollment rates, but they were also the most employable of the three groups; enrollees here tended to receive on-the-job training, job search assistance, or both. As intended, the other services subgroup included individuals with very diverse backgrounds and service needs. Some adults in this group were also among the most employable, and so a substantial number of them were enrolled in job search assistance only, whereas youths in this group tended to be among the least job-ready, and so they tended to be enrolled in basic education and other services intended to address their lack of work experience.

4

Adult Women: JTPA Impacts at 18 Months

THIS chapter presents our findings on the impacts of JTPA Title II-A on the earnings, employment, and high school attainment of adult women at the 16 study sites. These impact estimates provide the first reliable evidence of the effectiveness of Title II-A for adult women. Moreover, they constitute the first major experimental evaluation of the effects of employment and training programs on women not receiving Aid to Families with Dependent Children (AFDC), who make up two-thirds of the women in our study sample.

The 6,607 adult women in this 18-month study sample were ages 22 or older at their random assignment, and like the adult men reported on in the next chapter, they had an average age of 33, with about 88 percent between ages 22 and 44 and less than 5 percent age 55 or older.[1] The women resembled the men in several other respects: over half were white, 31 percent were black, and 11 percent were Hispanic. Over two-thirds had a high school credential upon application to JTPA, and most (86 percent) had worked before, although few women or men were employed when they applied to the program.[2]

Unlike the men, however, nearly half of the women had worked less than 13 weeks in the past 12 months, and over a third had earned low wages (less than $4.00 an hour) in their most recent job. Thus, the average earnings of adult women in the year before their application to

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[1] The estimated impacts on earnings and employment reported in this chapter are based on the 5,826 women who responded to the First Follow-up Survey (88.2 percent of the 18-month study sample) and on 648 nonrespondents for whom earnings data from state unemployment insurance agencies were available. Appendix D describes the procedures used to combine the respondent and nonrespondent data.

[2] The information on baseline characteristics presented in this and the next paragraph is based on Background Information Form responses from all women in the 18-month study sample. For more detail see Exhibit 3.13 in Chapter 3.

JTPA ($2,359) were much lower than those of adult men ($3,996). As might be expected, the women were also much more likely to have a child of their own in the household (70 percent of the women as opposed to 33 percent of the men) and to be receiving public assistance, including food stamps (60 percent of the women versus 37 percent of the men). As a result of all these differences, the women in the sample were probably less readily employable than the men.

This chapter begins by presenting the estimated average impacts of JTPA on adult women overall. The first main section examines impacts on earnings, and the second, impacts on employment. Using the findings from these two sections, we then decompose the estimated impacts on earnings into effects on the percentage employed, weeks worked if employed, hours worked per week worked, and earnings per hour worked—what we term the *components of earnings*. The estimates in this third section offer insight into such issues as whether the program led to more stable jobs, more full-time jobs, or jobs paying higher wage rates.

The fourth and fifth sections focus on certain subgroups of women of interest to policymakers and program planners. The fourth section looks at women recommended for each of the three broad clusters of program services we have defined as *service strategies*: classroom training, on-the-job training/job search assistance (OJT/JSA), and other services. The estimated impacts on earnings by service strategy subgroup offer evidence of the effects of different clusters of JTPA activities on the subgroups of women whom program staff deemed appropriate for each cluster.

In the fifth section we turn to other selected subgroups, defined by characteristics, such as barriers to employment facing women, that may have affected their ability to benefit from the program. Here we also present estimates that control for the distribution of each key subgroup across sites and for its distribution across both sites and service strategies—to help distinguish between effects across subgroups that reflect differences in the way subgroups are distributed across sites or service strategies and those that reflect differences in the characteristics of the women themselves.

In the chapter summary we review the findings, with reference to results from previous experimental studies of the effects of employment and training programs serving women.

The impact estimates presented throughout this chapter were derived by the methods outlined in Chapter 2. However, unlike the estimates for adult men and youths in the following chapters, these for adult women were adjusted for survey nonresponse bias, because our tests for nonresponse bias using earnings data from state unemployment insurance (UI) records indicated a small but nonnegligible bias in the impact estimates for this target group. For a full discussion of the methodologies employed in all these procedures, see Appendix D.

Impacts on Earnings: Adult Women Overall

This section presents estimates of program impacts on the earnings of adult women. These estimates serve as summary measures of the overall effects of JTPA Title II-A on this target group and as a benchmark for comparing the estimates for subgroups of women presented later in the chapter.

This section begins by contrasting the monthly earnings trends of treatment group and control group members over the 18-month follow-up period beginning at random assignment. It then describes the pattern of the treatment group's enrollment in JTPA over the same time period, to distinguish between the in-program and post-program periods for the group as a whole. The section proceeds by detailing our estimates of program impacts on assignees and enrollees and ends by examining impacts on the distribution of earnings.

MONTHLY EARNINGS TRENDS: TREATMENT GROUP AND CONTROL GROUP

Exhibit 4.1 shows the earnings experience of treatment group and control group members over the first 18 months after their random assignment.[3] As shown, the *treatment group*—those women in the sample who had access to JTPA—consistently had higher average monthly earnings than the *control group*, whose members were denied access to the program for the purposes of the experiment.

The estimates underlying the earnings curve for the treatment group represent an *outcome* of JTPA—what the treatment group earned after its members gained access to JTPA: from $268 in the first follow-up month to $521 in the eighteenth.[4] This information does not tell us what the treatment group would have earned without access to JTPA, which is what we must know to calculate the *impact* estimates we will present shortly.

It is the control group curve that provides this information. Average earnings of control group members ranged from $261 in the first follow-up month to $478 in the eighteenth.[5]

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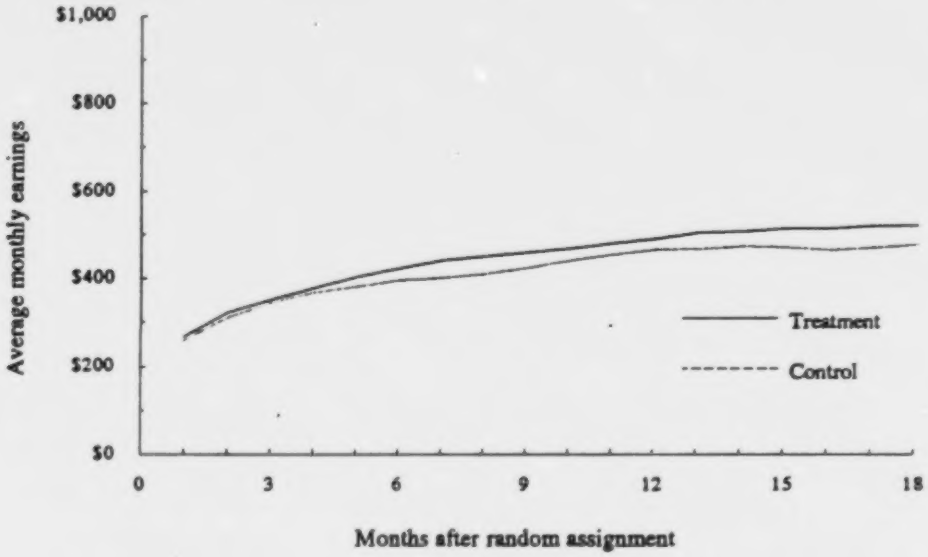
[3] Throughout this chapter, earnings and impact estimates are expressed in nominal dollars. The follow-up period varied across individuals, beginning as early as November 1987 and ending as late as December 1990.

[4] We used ordinary least squares regression procedures to increase the statistical precision of these estimates, as described in Appendix D.

[5] The earnings estimates shown in Exhibit 4.1 and subsequent exhibits include wages paid to JTPA participants in on-the-job training positions. During the 18-month follow-up period the program reimbursed employers a total of about $650 per adult female *OJT participant*. Among all adult women in the sample OJT reimbursements totaled about $80 per *treatment group member* over the 18-month follow-up period.

Exhibit 4.1 Average Monthly Earnings: Treatment Group and Control Group



Sources: Estimates based on First Follow-up Survey responses and earnings data from state unemployment insurance (UI) agencies.
Notes: Sample size, treatment group = 4,376, control group = 2,098. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

These point estimates actually provide two crucial pieces of information. First, in representing our estimates of what the treatment group would have earned, on average, without access to JTPA, they serve as the basis for the treatment-control group comparisons of the experimental impact analysis. And second, they demonstrate the importance of making these comparisons in evaluating employment and training programs, since they indicate that, even without access to JTPA, control group members increased their average monthly earnings substantially over the course of the follow-up period.

Control group members were excluded from JTPA services only; they were free to participate in any non-JTPA employment and training services available in the community. The control group earnings curve shown in Exhibit 4.1 therefore reflects the effects of any non-JTPA services received by control group members. Thus, the benchmark against which we measure the impact of JTPA services is not the total absence of any employment and training services but the level of non-JTPA services available in the community. That is, we measure the *incremental* effect of JTPA, over and above the effects of other available employment and training services.[6] (Later in this chapter we present estimates of services received by both treatment group and control group members.)

ENROLLMENT PATTERNS OVER TIME: TREATMENT GROUP

In the early part of the follow-up period the differential between treatment group and control group earnings may have been affected by the treatment group's participation in JTPA, in several different ways. On the one hand, enrollment in occupational skills training or basic education may have actually delayed employment for some treatment group members early in the follow-up period, causing their earnings to be lower than those of their control group counterparts. On the other hand, enrollment in job search assistance or on-the-job training may have led to faster job placements for other treatment group members, causing their earnings to surpass those of their control group counterparts in the early months of the follow-up period.

Any differential between treatment group and control group earnings later in the follow-up period, however, should be relatively free of these conflicting effects of program participation and should therefore reflect the post-program effects of JTPA. Exhibit 4.2 allows us to establish this distinction between *in-program* and *post-program* periods, by showing the percentage of all those assigned to the treatment group who were enrolled in JTPA Title II-A in each month of the follow-up period.[7]

Among adult women enrollment rates of the treatment group dropped from 58 percent in the first follow-up month to only 11 percent in the eighteenth.[8] Throughout this report we will use the first month in which less than 15 percent of the treatment group was still enrolled in JTPA as the beginning of the post-program period; thus, the post-program period for adult women began in month 10. The estimated impacts for the last eight months of the follow-up period may therefore be viewed as primarily *post-program impacts*. By referring back to Exhibit 4.1, we can see the positive treatment-control group differential in earnings persisted well after most treatment group members had left the program.

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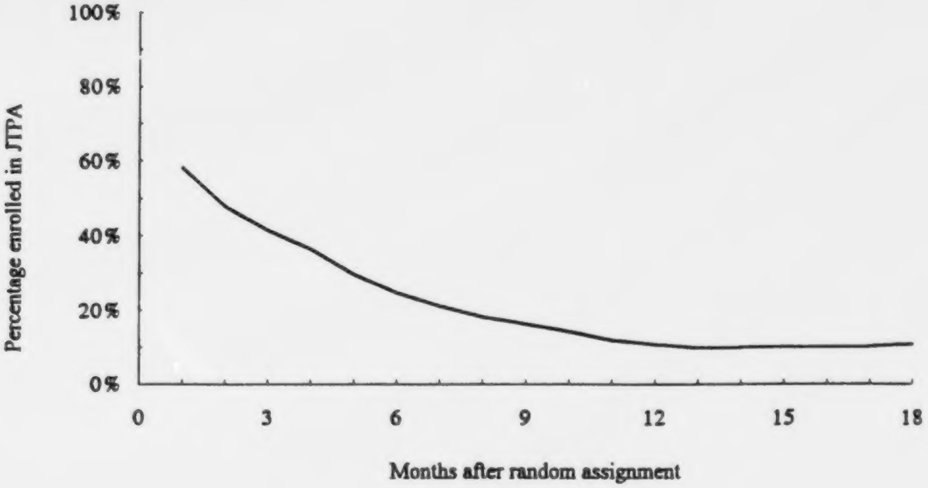
[6] In the benefit-cost analysis to be included in the final report, we will also measure the incremental *costs* of JTPA, for comparison with these incremental program impacts.

[7] Exhibit 4.2 is based on data from SDA records that somewhat overstate the number of persons still in the program at any given time because the data are missing some termination dates. Thus, this graph serves as an upper bound on the percentage of the treatment group still in the program in any given month; and the extent to which the graph overstates the actual enrollment rates is higher in the later months.

[8] Although 65 percent of the adult female treatment group was enrolled in JTPA Title II-A at some point during the follow-up period, only 58 percent was enrolled in the first follow-up month, because some treatment group members enrolled later.

Exhibit 4.2 Percentage Enrolled in JTPA Monthly: Treatment Group



Source: Unadjusted frequencies based on enrollment and tracking data from the 16 service delivery areas (SDAs).
Note: Sample size, treatment group = 4,465.

IMPACTS ON EARNINGS: JTPA ASSIGNEES AND ENROLLEES

In the simplest terms our estimates of average program impacts on earnings equal the difference between the average earnings of treatment group members and the average earnings of control group members. Exhibit 4.3 serves to illustrate our derivation of these estimates.

First note that we have shifted from monthly to quarterly earnings. The first column in the exhibit shows estimates of the average quarterly earnings of the treatment group, which rose from $942 in the first follow-up quarter to $1,555 in the last, and average total earnings over all quarters in the follow-up period, which were $8,027. Column 2 shows the corresponding estimates for the control group, which rose from $916 in the first quarter to $1,414 in the last, for a total of $7,488 over the full period.

Column 3 presents the estimated impacts: the differences between columns 1 and 2. Significance tests indicate that these differences were statistically significant in the last five quarters of the follow-up period and for the period as a whole (as indicated by the asterisks by these estimates). This means we have confidence that JTPA increased the earnings of adult women in these time periods; in other words, it is unlikely that these treatment-control group

Exhibit 4.3 Impacts on Earnings: Treatment Group

	Mean earnings		Impact on treatment group earnings	
Period	Treatment group (1)	Control group (2)	In $ (3)	As % of (2)[a] (4)
Quarter 1	$ 942	$ 916	$ 26	2.9%
2	1,205	1,145	60*	5.2
3	1,353	1,236	118***	9.5
4	1,442	1,363	78**	5.8
5	1,529	1,413	116***	8.2
6	1,555	1,414	141***	9.9
All quarters	8,027	7,488	539***	7.2

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.
Notes: Sample size, assignees = 4,376; control group = 2,098. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).
a. Significance levels for this column are identical to those in column 3.

differences in earnings are due to random sampling error. The estimated effect over the 18-month period was an earnings gain of $539. Column 4 expresses the dollar estimates in column 3 as a percentage of the control group mean. For simplicity's sake we do not show significance levels in this column, since they are the same as those in the corresponding rows of column 3.[9]

The impact estimates presented in columns 3 and 4 of Exhibit 4.3 measure the *average* effect of assignment to the treatment group on *all* treatment group members, regardless of their participation in JTPA. We therefore refer to these estimates as *impacts per JTPA assignee*. These estimates provide the most direct, reliable experimental evidence of the impacts of JTPA and, as such, are the estimates on which we will focus throughout this report.

But as shown in the previous chapter, not all JTPA assignees (treatment group members) actually enrolled in JTPA. It would therefore be of interest to know the impact of the program on only those who did enroll. Our estimates of *impacts per JTPA enrollee* adjust the estimated impacts per assignee to account for the fact that 35.4 percent of adult female treatment group members did not become enrolled in JTPA and for the fact that 2.9 percent of adult female control group members did become enrolled (despite the experiment's embargo on their participation).[10]

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[9] To increase the statistical precision of these estimates, we used ordinary least squares regressions. This reduced the standard errors of the impact estimates but did not appreciably affect the point estimates, because the average values of the independent variables (mainly the baseline characteristics of the treatment and control groups) were virtually the same for the two groups. See Appendix D for a full description of these procedures.

[10] Appendix D details the procedures used for these adjustments.

The adjustment for the treatment group's enrollment rate requires that we assume there was no JTPA impact on those members of the treatment group who did not formally enroll in the program. If some nonenrollees did experience positive effects from the program, the estimates of impacts per enrollee would overstate the impact on enrollees, because the adjustment would attribute these nonenrollee impacts to the enrollees.[11]

We explore this issue in Appendix F, which presents the results of a special study of JTPA services received by a subsample of nonenrolled treatment group members. These findings suggest that roughly half of all adult female nonenrollees in the 18-month study sample received some form of JTPA service after their random assignment, in most cases job search assistance or referral to an employer for a possible on-the-job training position. Since these services were limited in scope and intensity, their impacts on nonenrollees were probably negligible. Nevertheless, we cannot be sure of this conclusion, and there is evidence to suggest the validity of this conclusion may vary among women recommended for different service strategies.

We therefore consider our inferred impacts per JTPA *enrollee* to be estimates of the upper bound on the size of the average program impact on enrollees, since they may spread the total impact over too few treatment group members—that is, only those who formally enrolled. In contrast, we consider our estimates of impacts per JTPA *assignee* to be estimates of the lower bound, since they spread the total impact over too many treatment group members; that is, they include some assignees who had no exposure to the program beyond the act of application.

Exhibit 4.4 displays the estimated impacts on quarterly earnings per assignee and per enrollee. The first three columns simply repeat the information about impacts per treatment group member (assignee) presented earlier in Exhibit 4.3. The first column shows estimated average earnings of the control group. Columns 2 and 3 repeat the dollar and percentage impacts per assignee. Column 4 presents the estimated impacts per enrollee, which are uniformly larger than the estimates per assignee by a factor of 1.62.[12]

As shown in the exhibit, the inferred impacts on the earnings of adult women who enrolled in JTPA ranged from $42 in the first quarter after random assignment to $228 in the sixth, for

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[11] It is possible that the program impact on nonenrolled treatment group members was the opposite of any impact on the enrollees. If this were so, which seems unlikely, the estimated impacts per enrollee would understate the magnitude of the average impact on enrollees.

[12] As explained in Appendix D, the adjustment factor used to derive impacts per enrollee from impacts per assignee is $1/(r-c)$, where r is the enrollment rate (the proportion of treatment group members who enrolled in JTPA) and c is the crossover rate (the proportion of control group members who were enrolled in JTPA). Since these two rates are fixed for any given group or subgroup, the ratio of impacts per enrollee to impacts per assignee is also fixed for any given group or subgroup. Thus, for example, for the adult female target group the impact per enrollee is 1.62 times the impact per assignee for all outcomes in all time periods.

Exhibit 4.4 Impacts on Earnings: JTPA Assignees and Enrollees

	Control mean	Impact per assignee		Inferred impact per enrollee, in $
		In $	As % of (1)	
Period	(1)	(2)	(3)	(4)
Quarter 1	$ 916	$ 26	2.9%	$ 42
2	1,145	60*	5.2	97
3	1,236	118***	9.5	190
4	1,363	78**	5.8	127
5	1,413	116***	8.2	188
6	1,414	141***	9.9	228
All quarters	7,488	539***	7.2	873

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, assignees = 4,376; control group = 2,098. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

a total earnings gain of $873 over the entire follow-up period. If treatment group nonenrollees experienced negligible program effects, the total impact per enrollee was probably close to this upper bound estimate. But if nonenrollees experienced a substantial positive effect from the JTPA services they received, the total impact per enrollee was probably closer to $539, the lower bound suggested by the total impact per assignee. Because of this uncertainty we did not attempt to calculate the statistical significance of the inferred impacts per enrollee in this column or elsewhere in this report.

IMPACTS ON THE DISTRIBUTION OF EARNINGS: JTPA ASSIGNEES

These average earnings gains for assignees and enrollees were not, of course, evenly distributed across all women. Some women may have gained substantially more than the average impact estimated, while others may have had small gains or even losses. We would like to know the distribution of program impacts on individual treatment group members. For example, does the $539 impact per assignee for the follow-up period as a whole represent a large impact on only a small percentage of the women or modest impacts distributed across the sample?

Because we cannot match up *individual* sample members in the treatment group with individuals in the control group, we cannot estimate the *distribution of impacts* on the earnings of individuals. We can, however, estimate program *impacts on the distribution of earnings* of the treatment group. Here we are asking how JTPA changed the shape of the

Exhibit 4.5 Impacts on the Distribution of Total 18-Month Earnings: JTPA Assignees

18-month earnings	*Assignees* *(1)*	*Control group* *(2)*	*Difference, in % points* *(3)*
$0	21.0%	23.5%	-2.5%**
$1 - $3,800	18.7	19.4	-0.7
$3,801 - $8,500	19.1	19.0	0.1
$8,501 - $14,300	20.2	19.1	1.1
> $14,300	21.0	19.0	2.0*
Chi-squared test of impact on entire distribution			*

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.
Notes: Sample size, assignees = 4,336; control group = 2,079. For the estimation procedure, see Appendix D.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (chi-squared test or two-tailed t-test).

earnings distribution. In other words, did the program increase the percentage of treatment group members with higher earnings or reduce the percentage with lower earnings?

Exhibit 4.5 shows the distribution of total 18-month earnings for the treatment and control groups (columns 1 and 2) and the program impact on the proportion of JTPA assignees whose earnings over the period were within each earnings category (column 3). The first earnings category in the exhibit is defined as those sample members with zero earnings over the period; the remaining four categories represent approximate quartiles of the earnings distribution of those control group members with positive earnings.[13]

As shown in column 3, which presents the differences between the distributions of the two groups, JTPA significantly reduced the percentage of assignees with zero earnings during the follow-up period by -2.5 percentage points. This finding implies that the program increased the proportion of women who found jobs, which is consistent with the estimated impacts on employment presented in the next section. The program also significantly increased the proportion of assignees in the highest earnings category, those earning more than $14,300 over the 18-month period. Unfortunately, we cannot tell where in the earnings distribution those assignees who would have had zero earnings in the absence of the program fell or where those whom the program moved into the top earnings category came from. Nevertheless, the test for the significance of the impact on the earnings distribution as a whole does tell us that the program did produce a change in the distribution.

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[13] The method by which the estimated impacts on the distribution were adjusted for survey nonresponse bias made it impossible to divide the control distribution into exact quartiles. See Appendix D for an explanation of this procedure.

Impacts on Employment: Adult Women Overall

Although the estimated impacts on total earnings are perhaps the best summary measures of the effect of JTPA Title II-A, it is also of interest to examine program effects on more detailed measures of labor market success. This section presents estimates of program impacts on three measures of employment—the *percentage employed*, the average number of *weeks worked*, and the average number of *hours worked*—during each follow-up quarter and for the 18-month period as a whole.

Exhibit 4.6 displays estimates based on these measures in three separate panels. The first panel indicates that JTPA significantly increased the proportion of adult women employed at some time during the third, fifth, and sixth quarters after random assignment and of those employed at some time during the follow-up period as a whole. Overall, the proportion of the treatment group ever employed during the follow-up period was 2.1 percentage points higher than it would have been in the absence of the program.[14] Among JTPA enrollees the proportion ever employed was an estimated 3.5 percentage points higher than it would have been without access to the program.

The program did not have a statistically significant effect on the average number of weeks or hours worked by assignees over the follow-up period as a whole. Although the estimated effects on average weeks (2.8 percent) and hours (3.7 percent) were roughly the same size as the effect on the percentage employed (2.8 percent), we cannot tell whether there was a true impact on the average number of weeks and hours worked because the impacts shown in these last two panels were estimated less precisely than those in the first panel.[15]

There were, however, significant positive effects on weeks and hours worked in selected quarters. The average number of weeks worked was 4.2 percent and 4.9 percent higher among assignees than control group members in the third and sixth quarters, respectively; and the average number of hours were between 4.7 percent and 6.5 percent higher among assignees in the third, fifth, and sixth quarters.

It is important to recognize that these estimated effects on the average number of weeks and hours of employment for all assignees may simply reflect the program's positive effect on the proportion of women employed (as shown in the first panel of the exhibit). Consider, for example, the following hypothetical case. Suppose that all women who would have worked without access to JTPA worked exactly the same number of weeks

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[14] Because the outcome shown in the top panel of Exhibit 4.6 is the percentage of the sample *ever* employed in the relevant time period (quarter or 18-month follow-up period), quarterly proportions do not sum or average to the proportion for the follow-up period as a whole.

[15] These impacts are estimated less precisely because weeks and hours employed have higher variance, relative to the estimated effect, than does the percentage employed.

Exhibit 4.6 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: JTPA Assignees and Enrollees*

		Impact per assignee		
Period	*Control mean* (1)	*In % pts., weeks, or hours* (2)	*As % of (1)* (3)	*Inferred impact per enrollee, in % pts., weeks, or hours* (4)
		Percentage employed		
Quarter 1	48.4%	0.0%	0.0%	0.0%
2	53.4	1.4	2.6	2.2
3	55.8	2.4*	4.3	3.9
4	59.7	0.8	1.4	1.3
5	59.3	3.3***	5.5	5.3
6	60.9	2.2*	3.6	3.6
Anytime during quarters 1 - 6	76.8	2.1**	2.8	3.5
		Weeks worked		
Quarter 1	5.1	0.0	-0.2%	0.0
2	6.0	0.1	0.9	0.1
3	6.3	0.3*	4.2	0.4
4	6.8	0.1	1.5	0.2
5	7.0	0.3	3.7	0.4
6	6.9	0.3**	4.9	0.5
All quarters	38.3	1.1	2.8	1.8
		Hours worked		
Quarter 1	183	-1	-0.5%	-2
2	226	3	1.3	5
3	232	12*	5.4	20
4	250	8	3.0	12
5	257	12*	4.7	20
6	254	17**	6.5	27
All quarters	1,403	52	3.7	84

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, assignees = 4,376; control group = 2,098. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

and hours as they would have, but JTPA found employment for some women who would not have worked in the absence of the program. The additional weeks and hours of work associated with this latter group would raise the average number of weeks and hours worked for all assignees, even though the program had no effect on the weeks and hours worked by those who would have been employed anyway. Here the impact on average weeks and hours worked is simply a reflection of the program's impact on the proportion of all assignees who were employed at some time in any given period.

In the next section we attempt to distinguish between the effects on average weeks and hours worked per assignee that merely reflect a higher average employment rate among all adult women assigned to JTPA and those that reflect *additional weeks* and *hours* of work for those women who would have worked without access to the program. We do this by shifting our measures from average weeks and hours worked by *all* assignees (including zeros for nonworkers) to average weeks and hours worked per week worked by only those *women who did work*.

Impacts on the Components of Earnings: Adult Women Overall

The analysis in this section provides more detailed measures of labor market success than those presented so far. Specifically, these measures enable us to estimate whether JTPA Title II-A led to more stable employment, to more full-time jobs, or to jobs that paid higher wage rates.

The analysis is based on the fact that average earnings can be decomposed as follows:

$$\frac{\text{earnings}}{\text{assignee}} = \frac{\text{workers}}{\text{assignee}} \times \frac{\text{weeks}}{\text{worker}} \times \frac{\text{hours}}{\text{week}} \times \frac{\text{earnings}}{\text{hour}}$$

Each of the four *components of earnings* in this relationship reflects a different aspect of labor market success. *Workers per assignee* reflects the ability of assignees to find jobs—the "pure" employment effect. *Weeks worked per worker* reflects both how quickly assignees found jobs and how long they held them, or the stability of their employment. *Hours worked per week worked* reflects the mix of workers' part-time, full-time, and overtime work, that is, whether they were more likely to find a full-time job. And *earnings per hour worked* reflects what workers were paid for the time they worked.

Appendix D explains how we estimated JTPA impacts on each of these earnings components, and Exhibit 4.7 presents our findings. Because the percentage impacts on the last three outcomes shown in this exhibit were calculated indirectly, no tests of significance are shown in this exhibit. The significance levels of the impacts on total earnings (column 1)

Exhibit 4.7 Percentage Impacts on Earnings and Its Components: JTPA Assignees

Period	*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
Quarter 1	2.8%	0.0%	-0.3%	-0.3%	3.4%
2	5.2	2.6	-1.6	0.4	3.9
3	9.5	4.3	0.0	1.1	3.9
4	5.8	1.4	0.2	1.5	2.7
5	8.2	5.5	-1.7	1.0	3.3
6	10.0	3.6	1.2	1.6	3.2
All quarters	7.2	2.8	0.0	0.9	3.4

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.
Notes: Sample size, assignees = 4,376; control group = 2,098. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is displayed as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

and workers per assignee (column 2) are the same as those in exhibits 4.4 (for earnings) and 4.6 (for percentage employed).

Note that the percentage impacts on the four components in each row sum to approximately equal the percentage impact on earnings in the same row. They therefore measure the relative contribution of each component to the estimated impact on earnings.

As shown in the last row of the exhibit, the 7.2 percent average program impact on assignees' earnings over the follow-up period as a whole (column 1) was primarily attributable to program impacts on the percentage of assignees who were employed some time during the period (column 2) and to impacts on the earnings per hour worked by those who were employed (column 5). Slightly under half of the overall impact on earnings was due to a 3.4 percent increase in hourly earnings while employed, and most of the remainder was due to an increase of 2.8 percent in the percentage who were employed. There was little or no effect on the average number of weeks worked by those who were employed or on the number of hours worked per week worked. Thus, JTPA appears to have increased the probability that women would be employed, and to have raised their hourly earnings when they did work, but it did not seem to lead to steadier employment or a greater likelihood of full-time work.

Caution must be exercised in interpreting the estimated impact on earnings per hour worked (column 5) since it may reflect program effects on the composition of the subgroup of women who were employed, in addition to—or instead of—a positive program impact on the hourly earnings of specific individuals. If, for example, the additional employment generated by the program was concentrated among women with high hourly earnings, the average hourly earnings calculated for all workers would increase even if the program had no effect on the hourly earnings of any individual worker. Thus, the gain in average hourly earnings shown in Exhibit 4.7 does not necessarily imply that JTPA increased the *wage rates* of individual workers.

Similarly, program-induced changes in the composition of the subgroup of women who worked may also be masking effects on the average number of weeks and hours worked by individual women. Thus, the finding of little or no effect on weeks or hours worked by those who were employed should not necessarily be interpreted as evidence of a lack of effect on the weeks or hours worked by specific workers.

The distinction between program effects on the composition of the subgroup that worked and effects on specific individuals is especially important in the case of the estimates of earnings per hour worked, because increases in the hourly earnings of specific individuals may be evidence of a program effect on productivity. In an attempt to separate compositional effects from effects on the hourly earnings of individual workers, we used nonexperimental estimation techniques to derive the impact of the program on "latent wage rates."

A *latent wage rate* is the wage rate that a worker could command in the market if she were employed. Since the latent wage rate is defined for both workers and nonworkers, estimated impacts on latent wage rates are free of any compositional effects that may be clouding our interpretation of impacts on the average observed wage rates of workers. Unfortunately, since the latent wage rates of nonworkers cannot be observed, they must be estimated with nonexperimental methods of unknown reliability.

Our analysis of latent wage rates is presented in Appendix G. The results of this analysis show no statistically significant effect of JTPA on the latent wage rates of adult women in the fifth and sixth quarters after random assignment, that is, during the post-program period when one would expect any such effect to have materialized. For reasons discussed in the appendix, however, the nonexperimental methods used in this analysis may not be reliable. Thus, there may be no fully satisfactory way to address this issue with the data on the present sample.

Impacts on Earnings, Employment, and Earnings Components: Adult Women Recommended for Each Service Strategy

The impact estimates presented in the preceding sections are averages for all adult women in the 18-month study sample. They therefore reflect the effects of JTPA Title II-A on a wide variety of individuals whom program staff recommended for a number of different employment and training services. This section focuses on the subgroups of women recommended for each of the three clusters of services, or *service strategies*, defined in Chapter 2: the classroom training, OJT/JSA, and other services strategies.

As we saw in Chapter 3, each of these service strategy recommendations resulted in a distinctly different mix of services received by women in each of these subgroups. We would therefore expect that impacts might vary systematically among the three service strategy subgroups. It is important to bear in mind, however, that the three groups differed not only in the *services* each received, but also in the kinds of *people* within the group. Thus, any differences in impacts across these subgroups cannot be attributed solely to differences in the kinds of services each subgroup received. In other words, one cannot view differences in impacts among the service strategy subgroups as an indication of what might happen if a particular group of people were shifted from one strategy to another. Instead, the impact findings for the three subgroups must be viewed separately—as indicating how well each strategy worked for the distinct group of people it actually served.

We begin by examining how the employment and training services received by women varied across service strategy subgroups. We then describe the differences in services received between the treatment group and control group within each subgroup. The cross-strategy comparisons recall the comparisons in Chapter 3 showing how the three subgroups differed on this dimension. The treatment-control group comparisons, on the other hand, reflect the *increment in services* attributable to JTPA—the *source* of the impacts within each subgroup. As part of this discussion we also introduce estimates of program effects on the attainment of a high school diploma or General Educational Development (GED) certificate associated with participation in a school or training program.

We then show how the three service strategy subgroups differed from one another in terms of the kinds of women in each, as measured by their baseline characteristics. The remainder of the section presents the estimated impacts on each service strategy subgroup, based on the same measures of earnings, employment, and the components of earnings as those in the preceding sections on adult women overall.

DIFFERENCES IN EMPLOYMENT AND TRAINING SERVICES RECEIVED: TREATMENT AND CONTROL GROUPS WITHIN EACH SERVICE STRATEGY SUBGROUP

As described in Chapter 2, the three service strategy subgroups were defined based on the services *recommended* for them, not the services they actually *received*, because there is no way to identify the control group members who would have received a particular service. Because service recommendations were made by program staff prior to random assignment, control group members *can* be matched to treatment group members on the basis of recommended services. As a result, we can obtain purely experimental estimates of JTPA impacts on each of the three subgroups.

Not all treatment group members received the services for which they were recommended. Among adult female treatment group members overall, 35.4 percent were never enrolled; by service strategy subgroup the percentages not enrolling were 27.2 percent for the classroom training subgroup, 44.6 percent for the OJT/JSA subgroup, and 37.6 percent for the other services subgroup (Exhibit 3.12 in Chapter 3). And some of those who were enrolled received services other than the primary service recommended for them.

Nevertheless, separating the sample on the basis of recommended services is useful to the extent that it creates groups that received distinctly different clusters of services, because it allows us to analyze the effects of different service strategies. As demonstrated in Chapter 3 (Exhibit 3.19), the three service strategy subgroups *did* receive distinctly different clusters of services.

It must be recognized that each service strategy subgroup received a *mix* of services, not a single, isolated service. This means that the impacts reported for each service strategy subgroup reflect the effects of the mix of services received by that subgroup, not the impact of a single specific service. More specifically, as explained earlier, our impact estimates represent the incremental effect of the difference between the mix of services received by the treatment group and the mix received by the control group.

As shown in Exhibit 3.19 in Chapter 3, 88.8 percent of the adult female *enrollees* in the classroom training subgroup received classroom training in occupational skills, basic education, or both. Likewise, 87.8 percent of those enrollees in the OJT/JSA subgroup received on-the-job training, job search assistance, or both. Finally, 82.3 percent of enrollees in the other services subgroup received one or both of job search assistance or miscellaneous services, the key categories of services provided to adults recommended for this strategy.

The *impact* of the program, however, does not depend solely on the JTPA services sample members received. Rather, the impact depends on the *difference* between the services received by those with access to JTPA and the services they would have received if they had been excluded from the program. That is, the counterfactual against which we evaluate JTPA is the services available elsewhere in the community, not a total absence of services. We represent the services treatment group members would have received without access to JTPA by the employment and training services received by the control group, whose members *were* excluded from JTPA.

Exhibit 4.8 displays estimates of this service difference for women recommended for each service strategy. The exhibit compares both the percentage of treatment group and control group members receiving each service (their likelihood of receiving the service) and the average number of hours of each service received (the *amount* of the service received).

Note that the estimates for different services were derived from different sources. There are several reasons for this. First, because *classroom training in occupation skills* and *basic education* are usually available from many non-JTPA providers, our only feasible source of information about these services was the First Follow-up Survey. But because survey respondents were likely to have difficulty distinguishing between JTPA-funded instruction and instruction funded by other public and not-for-profit programs, we did not attempt to make this distinction in the survey; we simply asked about employment and training services received from any provider. In general, however, we would expect most (but not all) of the services received by the treatment group to be JTPA services and virtually all of the services received by the control group to be non-JTPA services.[16]

Second, we could not measure receipt of *on-the-job training* or *work experience* in the survey, because respondents were likely to have difficulty distinguishing these positions from regular jobs. We therefore used enrollment and tracking data from the 16 JTPA service delivery areas (SDAs) to estimate receipt of these services by both the treatment group and the control group. Since these two services are usually *not* offered by non-JTPA providers, the treatment-control group difference in receipt of these services is probably a reasonably reliable indicator of the difference in service receipt.

A more serious omission results from the fact that the survey could not measure receipt of *job search assistance* over the course of the full follow-up period. The research team assumed that respondents simply would not be able to recall their receipt of this service—which is usually both brief and nonintensive—as long ago as 18 months before the interview. And since many control group members *were* likely to receive job search assistance from non-JTPA providers, the treatment-control group difference in receipt of this service from JTPA only would not be a reliable indicator of a service receipt difference. Finally, we also did not collect survey data on the services classified as *miscellaneous*, because of both potential recall problems and the difficulty of defining these services, which were much more numerous and varied than the examples listed in footnote *c* of the exhibit.

In summary, then, we were able to obtain reasonably complete survey data on receipt of classroom training in occupational skills and basic education from any service provider, for

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[16] The expectation regarding the treatment group is based on a comparison of the survey-based data and the enrollment and tracking data from the 16 service delivery areas reported in Chapter 3. The expectation regarding the control group is based on the fact that only 2.9 percent of the adult female control group enrolled in JTPA during the 18-month follow-up period.

Exhibit 4.8 Receipt of Employment and Training Services: Treatment Group and Control Group, by Service Strategy Subgroup

	Percentage receiving service			*Mean hours of service per sample member*		
Specific program service	*Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Treatment group (4)*	*Control group (5)*	*Difference, in hours (6)*
			Classroom training subgroup			
Classroom training in occupational skills[a] +	48.6%	28.7%	19.9%	351	242	110
Basic education[b] +	11.2	7.5	3.7	39	30	9
On-the-job training ++ (JTPA only)	3.3	0.2	3.1	27[c]	2[c]	25[c]
Work experience ++ (JTPA only)	3.9	0.0	3.9	23[c]	0[c]	23[c]
Job search assistance +++ (JTPA only)	17.1	–	–	–	–	–
Miscellaneous[d] +++ (JTPA only)	11.3	–	–	–	–	–
			OJT/JSA subgroup			
Classroom training in occupational skills[a] +	11.8%	12.0%	-0.2%	53	55	-2
Basic education[b] +	4.5	4.2	0.3	13	5	7
On-the-job training ++ (JTPA only)	28.5	0.7	27.8	107[c]	3[c]	104[c]
Work experience ++ (JTPA only)	2.6	0.0	2.6	13[c]	0[c]	13[c]
Job search assistance +++ (JTPA only)	26.5	–	–	–	–	–
Miscellaneous[d] +++ (JTPA only)	5.8	–	–	–	–	–

(Continued)

both the treatment group and the control group. We were also able to obtain SDA data on receipt of on-the-job training and work experience from JTPA providers, which serve as a fairly reliable source for the treatment-control group comparisons. But for receipt of job search assistance and miscellaneous services we report estimates only for the treatment group, because there was no reliable source of information on the control group's receipt of these last two categories of program services.

Before turning to the findings in Exhibit 4.8, note that the last three columns measure treatment-control group differences in the *amount* of services received as the average number of hours received by all sample members in the subgroup, including those who did *not* receive

Exhibit 4.8 Receipt of Employment and Training Services: Treatment Group and Control Group, by Service Strategy Subgroup (continued)

	Percentage receiving service			*Mean hours of service per sample member*		
Specific program service	*Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Treatment group (4)*	*Control group (5)*	*Difference in hours (6)*
			Other services subgroup			
Classroom training in occupational skills[a] +	19.1%	16.8%	2.3%	103	85	18
Basic education[b] +	10.7	7.6	3.1	33	26	7
On-the-job training ++ (JTPA only)	5.5	0.2	5.3	37[c]	2[c]	35[c]
Work experience ++ (JTPA only)	2.7	0.0	2.7	18[c]	0[c]	18[c]
Job search assistance +++ (JTPA only)	23.4	–	–	–	–	–
Miscellaneous[d] +++ (JTPA only)	31.5	–	–	–	–	–

Sources:

+ Unadjusted frequencies in this row are based on First Follow-up Survey data on receipt of the service from any provider.

++ Unadjusted frequencies in this row are based on enrollment and tracking data from the 16 SDAs, the best available data on receipt of this service. Although the data are for JTPA Title II-A-funded services only, this service is typically not funded by non-JTPA providers.

+++ Unadjusted frequencies in this row are also based on SDA enrollment and tracking data. Comparable data on receipt of this service from other providers were not available; nor were comparable data on receipt by control group members.

Notes: Sample size, classroom training subgroup: treatment group = 1,916, control group = 931; OJT/JSA subgroup: treatment group = 1,538, control group = 749; other services subgroup: treatment group = 922, control group = 418. Because of missing data, sample sizes for services calculated from different data sources may vary. Tests of statistical significance were not performed for this exhibit.

[a] Lasting longer than one week.

[b] Lasting longer than one week. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

[c] Hours, assuming a full-time job at 40 hours per week.

[d] "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

services. To calculate an estimate of the average number of hours of receipt for only those sample members who actually received a service *(service recipients)*, simply divide the hours for all treatment *or* control group members in the subgroup (column 4 *or* 5) by the proportion of the treatment group *or* control group members in that subgroup who received the service (column 1 *or* 2). Finally, also note that one cannot sum the percentages receiving services in each column because individual sample members might have received more than one service.

The Classroom Training Subgroup. Although *all* women in the classroom training subgroup were recommended for classroom training in occupational skills, only 48.6 percent of the treatment group reported receiving this service, as shown in the first panel of Exhibit 4.8.[17] More than a quarter of the corresponding control group reported receiving this service. Neither of these outcomes should be surprising.

Within the treatment group the principal reason why some women did not receive the services recommended for them was that 27 percent never enrolled in JTPA after gaining access to the program (see Exhibit 3.12 in Chapter 3). As noted at the start of this discussion, we expected that a number of control group members would receive employment and training services; by virtue of their application to JTPA, control group members demonstrated their motivation to seek out these services. In the case of the classroom training subgroup, the estimate for the control group in column 2 of the exhibit means that over a quarter of the control group succeeded in finding instructional services from other providers.

In fact, most communities have numerous providers of occupational skills training and basic education, many of which are subsidized by public funds. For example, community colleges offer a wide variety of vocational and technical courses at heavily subsidized tuition rates. And the same federal student aid programs that JTPA often taps to help support its trainees are available to students taking courses on their own, at either community colleges or private technical schools. State vocational education programs provide a variety of courses on a subsidized basis as well, and local public school systems and community-based organizations offer basic education classes for low or no fees.

Thus, with respect to occupational skills training and basic education, JTPA is not "the only game in town." The estimates in the first panel of Exhibit 4.8 demonstrate, however, that the program did increase the rate of receipt of these services. In the classroom training subgroup treatment group members were more likely than the control group to receive classroom training in occupational skills (48.6 percent vs. 28.7 percent) and were also somewhat more likely to receive basic education (11.2 percent vs. 7.5 percent).

In the classroom training subgroup, the average difference in the *amount* of service received between all members of the treatment group and the control group (column 6) was 110 additional hours for classroom training in occupational skills and 9 additional hours for basic education—that is, additional hours of service receipt attributable to the treatment group's having access to JTPA.

These relatively modest treatment-control group differentials in the amount of classroom training received per assignee reflect two factors noted earlier. First, not all treatment group members in the classroom training subgroup actually received classroom training. Second, a number of control group members in this subgroup received classroom training from non-JTPA providers.

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[17] The survey-based estimates of receipt of classroom training in occupational skills and basic education are somewhat inconsistent with SDA data on service receipt shown in Chapter 3, and they may in fact *understate* the incidence of these services. An analysis of these inconsistencies will be presented in our forthcoming final report.

These treatment-control group differentials in service receipt were largely a consequence of the differences in the likelihood of receiving each service, reported in the first three columns, and not primarily a consequence of a difference in the amount of service per service recipient. To see this, note that among *service recipients only*, the average hours of receipt of classroom training in occupational skills were similar for the treatment group, at 722 hours, and the control group, at 843 hours.[18] Likewise, among *recipients* of basic education, the hours of service receipt were also similar: 348 hours for the treatment group, and 400 hours for the control group. It is, of course, possible that the quality of the instruction received from non-JTPA providers was different from that received from JTPA.

Finally, note that 17.1 percent of the treatment group in this service strategy subgroup received job search assistance, and 11.3 percent received miscellaneous services, the last two categories in the panel.

The OJT/JSA Subgroup. In the OJT/JSA subgroup 28.5 percent (second panel of Exhibit 4.8) of the treatment group received on-the-job training and 26.5 percent received job search assistance. Again, the main reason why some women did not receive one of these two services was that 44.6 percent of the treatment group in this service strategy never enrolled in JTPA during the follow-up period (Exhibit 3.12 in Chapter 3).

As expected, the likelihood of receiving on-the-job training was higher for women in the OJT/JSA subgroup than for those in the other two service strategy subgroups. The likelihood of receiving job search assistance was also higher for the OJT/JSA subgroup than for the classroom training subgroup, although it was only slightly higher than that for the other services subgroup.

Regarding treatment-control group differences, recall that we do not have data on receipt of on-the-job training from non-JTPA providers. But since few of those providers offer OJT, the treatment-control group differences we report from JTPA providers are probably reasonably reliable indicators. If so, these estimates suggest that in the OJT/JSA subgroup treatment group members were more likely than control group members to receive on-the-job training (28.5 percent versus 0.7 percent). Column 6 indicates that JTPA provided an additional 104 hours of OJT, averaging over all treatment group members. This relatively small treatment-control group differential in the amount of OJT received per assignee reflects the fact that not all assignees in the OJT/JSA treatment group received OJT.

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[18] Again, these numbers are derived by dividing the average number of hours of service receipt per treatment or control group member (column 4 or 5) by the corresponding proportion receiving the service (column 1 or 2, divided by 100).

Among service recipients only, the average number of hours of OJT receipt was 429 (3 divided by 0.7 percent) for the control group and 375 (107 divided by 28.5 percent) for the treatment group. Thus, similar to the case for adult women in the classroom training subgroup, the positive treatment-control group difference in hours of OJT service shown in column 6 was almost exclusively the consequence of the difference in the likelihood of receiving OJT, and not of a difference in the amount of the service received by those who actually did receive it.

The treatment-control group differences in the receipt of classroom training and basic education were small in the OJT/JSA subgroup. Recall that data limitations precluded our calculating estimates of differences in the receipt of the last two categories, job search assistance and miscellaneous services.

The Other Services Subgroup. Because of the same data limitations we could not produce treatment-control group comparisons for these last two categories, the two main types of services received by adults in this subgroup. We do know, however, that 62.4 percent of the treatment group in the other services subgroup of adult women ultimately enrolled in JTPA (Exhibit 3.12 in Chapter 3) and that 82.3 percent of those who enrolled received job search assistance, miscellaneous services, or both (Exhibit 3.19).

The last panel in Exhibit 4.8 also indicates that although few members of the treatment group in the other services subgroup received on-the-job training or work experience, 19.1 percent received classroom training in occupational skills, and 10.7 percent received basic education. In the four categories of services for which we can calculate the difference in service receipt between the treatment group and the control group, the differences were small in absolute terms. The service differential in the case of basic education is a point that we will address further in the section on high school attainment below.

Summary of Differences in Service Receipt. The preceding comparisons of service receipt across service strategy subgroups and between the treatment and control groups within each strategy can be summarized as follows:

- Many women in the treatment group did not receive the primary service recommended for them, either because they were never enrolled in JTPA or because they received a service other than the primary one recommended for them.

- Nevertheless, the three service strategy subgroups, which were formed on the basis of service recommendations by JTPA intake staff, do appear to represent distinctly different clusters of services actually received and are therefore useful for analyzing the effects of alternative service strategies.

- Within each service strategy subgroup most women who enrolled in JTPA received one or both of two key services, which varied by service strategy: In the classroom training subgroup these were classroom training in occupational skills and basic education; in the OJT/JSA subgroup they were on-the-job training and job search assistance; and in the other services subgroup they were job search assistance and miscellaneous services. In all three subgroups the main reason why a portion of the treatment group did not receive one of the two key services in the strategy was failure to enroll in JTPA.

- The services actually received by each service strategy subgroup are consistent with the recommendations of intake staff to a considerable extent, but they differed in some important ways. Most important was the finding that only 28.9 percent of adult female treatment group members recommended for OJT (the OJT/JSA service strategy) actually received that service. Because 27.0 percent of adult female treatment group members recommended for OJT received job search assistance, it is most appropriate to characterize the service strategy as one based on placement in employment, with or without subsidized training.

- Treatment-control group differences in the average *amount* of service received were relatively modest, where those differentials could be measured. The average amount of additional occupational skills training received by treatment group members in the classroom training subgroup was only 110 hours. Similarly, the treatment-control group difference in receipt of OJT by women in the OJT/JSA subgroup was only 104 hours. These modest service differentials reflect the fact that not all treatment group members received these services and, in the case of occupational skills training, that some control group members received the service.

Because of the marked difference in the nature of the services received across the three subgroups, it is important to recognize that the costs of the three service strategies are also likely to have been quite different. Thus, the differences in impacts we report later in this section may not correspond to differences in cost-effectiveness. Our forthcoming final report will present a benefit-cost analysis that takes these differences in costs into account.

Impacts on High School Attainment. As part of the training described in this section, some of the women in the sample achieved a high school diploma or General Educational Development (GED) certificate. Exhibit 4.9 shows the impact of JTPA on the attainment of a high school credential by women who reported participating in school or training at some time during the follow-up period. We term such credentials "training related." Treatment and control group attainment rates are shown for adult women overall and for high school dropouts only within each service strategy subgroup. The treatment-control group differ-

Exhibit 4.9 Impacts on Attainment of a Training-Related High School Diploma or GED Certificate: JTPA Assignees Overall and High School Dropout Subgroup, by Service Strategy Subgroup

		Percentage attaining a training-related high school credential[b]		
	Sample size[a] *(1)*	*Assignees (2)*	*Control group (3)*	*Difference, in % pts. (4)*
		Classroom training subgroup		
Full sample	2,390	7.3%	2.9%	4.4%***
High school dropouts	605	29.2	11.3	17.9***
		OJT/JSA subgroup		
Full sample	1,955	2.6	3.5	-0.9
High school dropouts	574	9.1	10.9	-1.8
		Other services subgroup		
Full sample	1,064	5.3	3.3	2.1**
High school dropouts	336	17.4	9.8	7.6**
		All subgroups		
Full sample	5,409	5.2	3.2	2.0***
High school dropouts	1,515	19.1	10.8	8.2***

Sources: Unadjusted frequencies based on Background Information Form responses and First Follow-up responses.

a. Treatment and control groups combined.

b. "Attainment of a training-related high school credential" is defined as the combination of having received some school or training service and having attained a high school diploma or General Education Development certificate at some time during the 18-month follow-up period.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

ences shown in column 4 measure the additional educational attainment attributable to having access to JTPA.

As shown in the last panel of the exhibit, only 5.2 percent of the treatment group overall and 3.2 percent of the control group overall reported receiving a training-related high school credential at some time during the follow-up period. Thus, even though the treatment-control difference is statistically significant, the impact of the program on educational attainment is quite small when measured as a proportion of all women. This is because 71.8 percent of the treatment group already had a high school diploma (see Exhibit 3.13 in Chapter 3).

Among high school dropouts only (those women without a high school diploma or GED certificate upon application), the impact of the program was more substantial. As shown in the last line of the exhibit, 19.1 percent of the high school dropouts in the treatment group gained a training-related high school credential during the follow-up period, whereas only 10.8 percent of the dropouts in the control group did. We can therefore conclude that 8.2 percent of the dropouts assigned to the treatment group received a high school diploma or GED certificate as a result of their access to JTPA.[19]

As might be expected, this effect was concentrated in the classroom training subgroup, in which women were the most likely to receive basic education and in which the treatment-control group difference in the likelihood of receiving basic education was the largest. In this subgroup 29.2 percent of high school dropouts in the treatment group received a training-related high school credential, whereas only 11.3 percent of the corresponding control group did. There was also a significant effect on educational attainment in the other services subgroup, which as we saw earlier, exhibited a treatment-control group difference in the likelihood of receiving basic education nearly as large as that in the classroom training subgroup. There was no effect on training-related high school attainment in the OJT/JSA subgroup, in which there was almost no treatment-control group difference in the receipt of basic education.

Differences in Baseline Characteristics across Service Strategy Subgroups: JTPA Assignees

As explained more fully in Chapter 2, JTPA staff recommended sample members for the three service strategies on the basis of individuals' work experience and educational needs as well as their personal preferences. Variations in impacts among the three service strategy subgroups will therefore reflect not only the differences in services received by these groups, but also differences in their personal characteristics.

Exhibit 4.10 shows selected baseline characteristics of all adult female assignees and those in each of the service strategy subgroups.[20] Although most of the differences in characteristics among the three subgroups were not large, it does appear that program staff tended to recommend the OJT/JSA service strategy for the most job-ready applicants. Assignees in the OJT/JSA subgroup were less likely than those in the other two subgroups, especially the classroom training subgroup, to be facing the barriers to employment represented by welfare receipt or limited recent work experience. They were also slightly older, on average, than assignees in the classroom training subgroup and were more apt to

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[19] The difference is 8.2 percent because of rounding.

[20] For a more detailed description of the baseline characteristics of these subgroups, see Bloom (1991). Note, however, that the data in Bloom (1991) cover all JTPA applicants randomly assigned to treatment or control status, whereas Exhibit 4.10 includes only the adult female treatment group in the smaller 18-month study sample. Appendix A in the present report compares the treatment and control groups in this sample.

Exhibit 4.10 Selected Baseline Characteristics: JTPA Assignees, by Service Strategy Subgroup

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
Ethnicity				
White, non-Hispanic	54.5%	49.2%	62.6%	52.1%
Black, non-Hispanic	30.8	33.8	27.5	30.1
Hispanic	11.4	13.0	7.6	14.1
American Indian or Alaskan Native	1.0	1.2	0.9	1.0
Asian or Pacific Islander	2.3	2.8	1.4	2.8
Barriers to employment				
Receiving cash welfare[a]	37.9%	47.2%	29.7%	32.3%
No high school diploma or GED certificate	28.2	26.2	28.6	31.9
Worked less than 13 weeks in past 12 months	53.8	57.4	49.1	53.8
Number of barriers				
None of the above	28.6	24.5	33.1	29.9
One of the above	34.5	33.0	36.1	34.9
Two of the above	27.8	31.6	23.9	25.9
All three of the above	9.2	11.0	6.9	9.2
Work and training histories				
Ever employed	85.6%	81.9%	90.5%	85.2%
Mean individual earnings in past 12 months	$2,362	$2,092	$2,665	$2,441
Hourly earnings in most recent job				
Never employed	14.4%	18.1%	9.5%	14.8%
Less than $4	34.3	32.5	38.5	30.8
$4 or more	51.3	49.4	52.0	54.4
Employed upon application	14.6	14.3	15.5	13.9
Previously received occupational training	46.1	46.6	45.4	46.3
Public assistance status				
Receiving any public assistance[b]	58.7%	64.6%	54.3%	53.6%
Receiving AFDC	33.8	43.6	25.2	27.4
Receiving food stamps	50.3	56.0	46.2	45.2
Receiving other public assistance[c]	18.4	19.9	16.4	19.0

(Continued)

Exhibit 4.10 Selected Baseline Characteristics: JTPA Assignees, by Service Strategy Subgroup (continued)

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
AFDC history				
Never AFDC case head	49.1%	41.6%	55.5%	53.9%
AFDC case head less than 2 years	23.1	24.8	21.7	21.7
AFDC case head 2 years or more	27.9	33.5	22.8	24.4
JTPA required for welfare, food stamps, or WIN program [d]	12.3%	12.2%	12.0%	13.2%
Household composition				
No spouse or own child present	22.5%	15.4%	25.4%	28.3%
Own child under age 4, no spouse present	20.1	22.7	17.6	16.1
Own child, none under 4, no spouse present	34.9	40.4	34.5	33.2
Spouse present, with or without own child	22.6	21.5	22.5	22.3
Family income in past 12 months				
< $3,000	30.8%	28.5%	33.3%	31.4%
$3,000 - $6,000	33.7	36.9	29.5	34.3
$6,001 - $9,000	16.1	16.7	16.0	15.1
> $9,000	19.4	18.0	21.3	19.2
Living in public housing				
Yes	11.8%	14.0%	10.6%	9.6%
No	88.2	86.0	89.4	90.4
Age at random assignment				
22 - 29	44.2%	47.0%	43.4%	39.7%
30 - 44	43.1	44.6	41.4	42.7
45 - 54	8.0	6.1	10.5	8.0
> 54	4.7	2.3	4.7	9.6
Mean	*33.3*	*32.0*	*33.8*	*35.2*
Sample size	*4,465*	*1,966*	*1,562*	*937*

Source: Unadjusted frequencies based on Background Information Form responses.

[a] AFDC, General Assistance, or other welfare except food stamps.

[b] "Any public assistance" includes the following sources of assistance: AFDC, food stamps, unemployment insurance, housing assistance, and other cash assistance.

[c] "Other public assistance" includes unemployment insurance, housing assistance, and other (non-AFDC) cash assistance.

[d] WIN is the federal Work Incentive program.

have worked before, and to have had higher earnings in the year preceding their application, than those in the other two subgroups.

Assignees in the other services subgroup were older, on average, than those in the other two subgroups. They were also slightly less well educated, but (perhaps because of their age) they were the least likely to have received a low wage (less than $4.00 hourly) on their most recent job. In other respects, the other services subgroup tended either to be intermediate between the classroom training and OJT/JSA subgroups (for example, in terms of ethnic mix, earnings in the past year, and family income) or to resemble the OJT/JSA subgroup (for example, in terms of household composition and public assistance status).

Because of these differences among the women in each of the three service strategy subgroups, one must be careful in comparing program impacts across the three groups. The impacts presented in the next subsections reflect the effects of the program on the kinds of people recommended for each subgroup. If the same service strategy were recommended for a different set of people, there is no guarantee that the same impacts would be obtained. Thus, although the analysis can identify a strategy (or strategies) that was working, or not working, for the group of people recommended for that strategy, we cannot tell whether the labor market outcomes of one subgroup could be improved by substituting a different set of services.

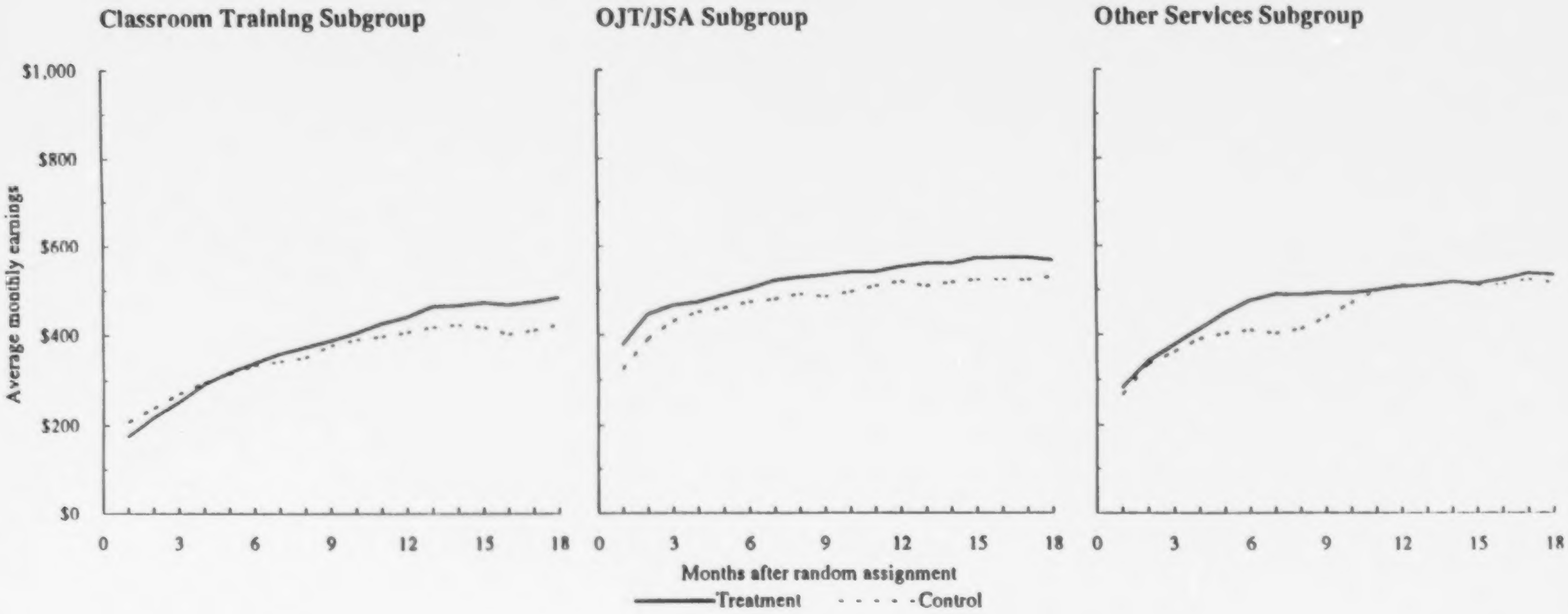
MONTHLY EARNINGS TRENDS: TREATMENT AND CONTROL GROUPS WITHIN EACH SERVICE STRATEGY SUBGROUP

As shown in Exhibit 4.11, the monthly earnings profiles of the treatment and control groups were markedly different across the three service strategy subgroups, both in the level of control group earnings and in the contrast between treatment group earnings and control group earnings.

In fact, the trends for the control groups in the three service strategies bear out our conclusion regarding the baseline characteristics of the three subgroups—that program staff assigned the more employable applicants to the OJT/JSA service strategy. Earnings in the absence of JTPA services, as measured by the control group level, were much higher for the OJT/JSA subgroup than for the classroom training control group in the first quarter after random assignment, and then continued to be higher over the entire follow-up period. Control group earnings in the other services subgroup were intermediate between these two subgroups.

In all three subgroups, earnings in the absence of JTPA services (the control group level) showed a pronounced upward trend throughout the follow-up period. Treatment group earnings were at or above the corresponding control group level throughout the follow-up

Exhibit 4.11 Average Monthly Earnings of Adult Women: Treatment Group and Control Group, by Service Strategy Subgroup



Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.
Notes: Sample size, classroom training subgroup: treatment group = 1,916, control group = 931; OJT/JSA subgroup: treatment group = 1,538, control group = 749; other services subgroup: treatment group = 922, control group = 418. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

period for two of the three subgroups—OJT/JSA and other services—and in the post-program period (months 10 through 18) for all three groups.

IMPACTS ON EARNINGS: JTPA ASSIGNEES AND ENROLLEES IN EACH SERVICE STRATEGY SUBGROUP

The patterns of quarterly impacts on earnings shown numerically in Exhibit 4.12 are highly consistent with what one would expect for each of the three service strategies. The *classroom training strategy* involves an initial investment of time in classroom training that can be expected to delay participants' employment. Once trained, however, participants should command higher earnings than they would have without training. Moreover, to the extent that classroom training improves trainees' human capital, these gains should persist for a relatively long time.

As shown in the top panel of the exhibit, this is exactly the pattern of Title II-A impacts estimated for the classroom training subgroup. The estimated impact on earnings grew continuously over the follow-up period, from a statistically significant loss of $-70 in the first quarter to statistically significant gains of $144 and $188 in the fifth and sixth quarters.

In contrast, the *OJT/JSA strategy* should involve no delay in employment. If anything, JTPA assignees recommended for this service strategy should show earnings gains early in the follow-up period, because of quicker job placements than they would have experienced in the absence of the program. And if participants in on-the-job training positions develop their human capital or learn job search skills that shorten any subsequent periods of unemployment, the early earnings gains should persist over the longer term.

This is in fact the pattern of effects shown in the middle panel of Exhibit 4.12. Assignees in the OJT/JSA subgroup experienced relatively uniform earnings gains throughout the 18-month follow-up period, with statistically significant gains in the range of $109 to $144 in all but the second quarter after random assignment.[21]

Finally, the *other services strategy* can be expected to have relatively rapid effects, because it comprises a variety of less intensive services, many of which are directed toward immediate employment. The expected duration of these effects is less clear, however, than it is for the other two subgroups. On the one hand, because many of these services involve little human capital development, one might expect these effects to be relatively short-lived.

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[21] As noted earlier, the earnings estimates include wages paid to JTPA participants in on-the-job training positions. The program reimbursed employers about $650 per adult female *OJT participant* during the 18-month follow-up period. Within the OJT/JSA subgroup, the reimbursement was about $150 per adult female *assignee* over the follow-up period.

Exhibit 4.12 *Impacts on Earnings: JTPA Assignees and Enrollees, by Service Strategy Subgroup*

		Impact per assignee		
Period	*Control mean (1)*	*In $ (2)*	*As % of (1) (3)*	*Inferred impact per enrollee, in $ (4)*
		Classroom training subgroup		
Quarter 1	$ 714	$ -70*	-9.8%	-100
2	938	5	0.5	7
3	1,066	52	4.9	74
4	1,189	79	6.6	112
5	1,253	144**	11.5	205
6	1,230	188***	15.3	268
All quarters	6,391	398	6.2	566
		OJT/JSA subgroup		
Quarter 1	$ 1,143	$ 144***	12.6%	273
2	1,379	81	5.9	153
3	1,449	129**	8.9	245
4	1,520	109*	7.2	207
5	1,546	142**	9.2	269
6	1,570	138**	8.8	263
All quarters	8,607	742**	8.6	1,409
		Other services subgroup		
Quarter 1	$ 960	$ 39	4.1%	67
2	1,198	132	11.0	223
3	1,248	220**	17.6	372
4	1,471	22	1.5	37
5	1,535	2	0.1	3
6	1,548	42	2.7	71
All quarters	7,960	457	5.7	773

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, classroom training subgroup: assignees = 1,916, control group = 931; OJT/JSA subgroup: assignees = 1,538, control group = 749, other services subgroup: assignees = 922, control group = 418. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

But several previous studies of low-intensity employment and training services for women receiving welfare indicated earnings gains that did not noticeably decline over the first 18 months or more (Gueron and Pauly, 1991). Most of the programs studied, however, were mandatory for AFDC applicants, recipients, or both and did not include other adult women. They therefore may not be comparable to JTPA, which serves a population of predominately voluntary participants, most of whom are not welfare recipients.

The third panel of Exhibit 4.12 indicates the pattern of JTPA impacts on earnings for the other services subgroup was one of relatively quick, but short-lived, effects. Estimated earnings gains sharply declined in the post-program period, from a statistically significant impact of $220 in the third quarter to a statistically insignificant gain of $42 in the sixth quarter.

Thus, the patterns of impacts on earnings over time suggest that simply comparing the effects on total earnings over the 18-month follow-up period for the three subgroups may be somewhat misleading. For the follow-up period as a whole, the other services subgroup had an average earnings gain that was somewhat larger than that of the classroom training subgroup ($457 versus $398). But the effect on the other services subgroup peaked midway into the follow-up period and had begun to disappear by the end, whereas the effect on the classroom training subgroup—and the OJT/JSA subgroup—was still large and significant in the last two quarters of the period.

If these patterns persisted beyond the 18-month follow-up period, the classroom training and OJT/JSA subgroups might witness substantially larger long-term effects than the other services subgroup. More speculatively, if the impacts of the classroom training strategy continued to grow over the longer term, while the impacts of the OJT/JSA strategy remained stable or declined, the classroom training strategy could well have the largest long-term effects. These issues will be addressed for a longer follow-up period in our forthcoming final report.

It is important to remember, however, that what matters from a policy perspective is not just the absolute size of the impacts on the service strategy subgroups, but the impacts relative to the costs of each strategy. Again, the final report will also present a benefit-cost analysis that compares the impacts of the three JTPA service strategies and their costs, to determine which one or ones were cost-effective.

Even then, however, we will only be able to say which service strategies were cost-effective for the individuals recommended for each one. There is no guarantee that a service strategy that has been shown to be cost-effective for one group will be cost-effective for a different group.

The fourth column of Exhibit 4.12 shows the impacts on earnings of the typical JTPA *enrollee* that can be inferred from the impacts per assignee in column 2 of the exhibit, if we assume that the program had no effect on treatment group members who did not enroll in JTPA. For the classroom training subgroup these impacts per JTPA enrollee were uniformly 37 percent larger than the impacts per assignee; for the OJT/JSA subgroup they were 80 percent larger; and for the other services subgroup, 60 percent larger. (The adjustment to the impact per assignee varies among subgroups because the JTPA enrollment rate, upon which the adjustment is based, varied by subgroup.)

It is important to bear in mind that the inferred impacts per enrollee shown in Exhibit 4.12 probably overstate somewhat the true impacts on JTPA enrollees. These estimates are based on the assumption that there was no impact on treatment group nonenrollees. As noted in Chapter 3, some nonenrollees received some JTPA service, however minimal that service, and therefore their earnings may have been affected by the program.

IMPACTS ON EMPLOYMENT: JTPA ASSIGNEES IN EACH SERVICE STRATEGY SUBGROUP

As we did for adult women overall, we now examine the JTPA effects on employment for women in each of the three service strategy subgroups. Exhibit 4.13 displays in columns the estimated program impacts per assignee on the *percentage employed*, the average number of *weeks worked*, and the average number of *hours worked*, with results for the three subgroups again displayed in panels.

Not surprisingly, the patterns of estimated effects over time for employment roughly parallel the patterns for earnings shown in the previous subsection. However, as shown in the top panel of the exhibit, the only statistically significant impacts on the employment of women in the *classroom training subgroup* were reductions in average weeks and hours of work of about half a week in the first quarter and an increase in average weeks worked of about half a week in the sixth quarter.

In the *OJT/JSA subgroup*, the percentage employed increased significantly in the fifth quarter only, and there were no significant effects on weeks worked. Average hours worked significantly increased in the first, third, and fifth quarters, however; and for the follow-up period as a whole average hours worked were nearly 100 hours higher than they would have been in the absence of the program.

Adult female assignees in the *other services subgroup* had significantly higher employment rates, average weeks worked, and average hours worked than the corresponding control group in the third quarter, where we earlier saw significant earnings gains. They also had significantly higher employment rates in the second and fifth quarters. For the follow-up

Exhibit 4.13 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: JTPA Assignees, by Service Strategy Subgroup*

	Percentage employed		*Weeks worked*		*Hours worked*	
Period	*Control mean* *(1)*	*Impact, in % pts.* *(2)*	*Control mean* *(3)*	*Impact, in weeks* *(4)*	*Control mean* *(5)*	*Impact, in hours* *(6)*
			Classroom training subgroup			
Quarter 1	39.6%	-2.1%	4.1	-0.4*	147	-24***
2	46.7	-1.0	5.1	-0.2	186	-14
3	49.7	1.0	5.5	0.0	203	-6
4	54.5	0.4	6.0	0.0	220	-1
5	54.4	2.5	6.4	0.1	230	3
6	55.5	2.5	6.1	0.4*	220	15
All quarters[a]	72.5	1.3	33.7	-0.1	1,222	-30
			OJT/JSA subgroup			
Quarter 1	60.1%	1.3%	6.2	0.3	234	23**
2	62.3	1.7	7.0	0.1	268	11
3	63.7	1.9	7.1	0.2	273	21*
4	65.7	2.2	7.6	0.0	288	13
5	66.0	3.4*	7.8	0.2	290	19*
6	67.4	1.7	7.8	0.0	290	15
All quarters[a]	82.8	2.4	43.8	0.7	1,655	97*
			Other services subgroup			
Quarter 1	47.0%	3.0%	4.8	0.5	172	7
2	52.3	6.5**	5.8	0.5	213	7
3	54.9	6.4**	6.1	1.0***	224	33**
4	60.5	-0.6	6.6	0.5	252	10
5	58.5	4.5*	6.9	0.7**	261	9
6	61.6	2.3	7.1	0.5	267	13
All quarters[a]	74.8	4.3*	37.2	3.7**	1,385	79

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, classroom training subgroup: assignees = 1,916, control group = 931; OJT/JSA subgroup: assignees = 1,538, control group = 749; other services subgroup: assignees = 922, control group = 418. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. For columns 1 and 2 ("percentage employed") this row shows the percentage of control group members who reported being employed at any time during the follow-up period and the estimated impact on this percentage for assignees, respectively.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

period as a whole this subgroup was significantly more likely to be employed at some time and, on average, worked 3.7 more weeks than they would have without access to JTPA.

As noted earlier, because these three employment outcomes are averages over all assignees in each subgroup, the estimated effects on each are not independent. In particular, any impacts on average weeks and hours employed for all assignees may simply be a reflection of a higher proportion of women working. The next subsection focuses on the average weeks worked and hours worked per week of only those assignees who worked, to distinguish the added hours and weeks of work generated by an effect on the employment rate alone and those generated by effects on the hours and weeks worked by those who were employed.

IMPACTS ON THE COMPONENTS OF EARNINGS: JTPA ASSIGNEES IN EACH SERVICE STRATEGY SUBGROUP

As shown earlier in this chapter, the average earnings of JTPA assignees can be decomposed into four multiplicative parts: the proportion of all assignees who were employed, the average number of weeks worked per worker, the average number of hours worked per week worked, and the average earnings per hour worked. The estimated impacts on these components of earnings tell us whether access to JTPA made it more likely that women would be employed at all and whether those who were employed were more likely to work more steadily, to have a full-time job, or to earn more per hour. Exhibit 4.14 presents these estimates as a percentage of the corresponding control group mean for each service strategy subgroup.[22]

Recall that because the percentage impacts on the four components in each row of the exhibit sum to approximately equal the percentage impact on earnings in the same row, they indicate the relative contribution of each component to that impact on earnings. The exhibit shows that the composition of the earnings gains differed substantially across the three service strategy groups.[23]

In the *classroom training subgroup* (top panel) the 6.2 percent impact on earnings over the entire follow-up period was predominately the result of an 8.9 percent impact on earnings per hour worked, which offset a small decrease in the duration of employment spells (as measured by average weeks worked per worker) and a small decrease in full-time work (as measured by average hours worked per week worked). For this subgroup the program had

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[22] See Appendix D for a description of the method by which the estimates were calculated.

[23] Again, we did not calculate statistical significance levels for the estimates in the earnings components exhibits, because the estimates in the last three columns were calculated indirectly.

Exhibit 4.14 *Percentage Impacts on Earnings and Its Components: JTPA Assignees, by Service Strategy Subgroup*

Period	*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
			Classroom training subgroup		
Quarter 1	-9.8%	-5.3%	-4.9%	-7.3%	8.0%
2	0.6	-2.1	-0.9	-4.8	9.0
3	4.9	2.0	-2.5	-2.4	8.0
4	6.6	0.7	0.1	-1.1	7.0
5	11.6	4.6	-2.7	-0.7	10.4
6	15.3	4.5	2.5	-0.1	7.8
All quarters	6.2	1.8	-2.1	-2.1	8.9
			OJT/JSA subgroup		
Quarter 1	12.6%	2.1%	2.5%	4.8%	2.6%
2	5.8	2.7	-1.9	3.5	1.5
3	8.9	3.1	0.2	4.1	1.3
4	7.2	3.3	-2.9	4.1	2.6
5	9.2	5.2	-2.5	3.9	2.4
6	8.8	2.5	-1.9	4.6	3.4
All quarters	8.6	2.9	-1.3	4.2	2.6
			Other services subgroup		
Quarter 1	4.1%	6.4%	3.1%	-4.9%	-0.2%
2	11.0	12.4	-3.9	-4.2	7.3
3	17.6	11.7	3.6	-0.9	2.5
4	1.5	-1.0	9.1	-3.6	-2.5
5	0.1	7.6	2.4	-6.2	-3.2
6	2.7	3.8	3.7	-2.6	-1.9
All quarters	5.7	5.8	3.9	-3.7	0.0

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, classroom training subgroup: assignees = 1,916, control group = 931; OJT/JSA subgroup: assignees = 1,538, control group = 749; other services subgroup: assignees = 922, control group = 418. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is calculated as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

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only a small positive effect on the percentage employed.[24] These results are consistent with the view that classroom training involves an initial investment of forgone employment and earnings that should then lead to higher rates of pay.

The 8.6 percent impact on the earnings of those in the *OJT/JSA subgroup* (which was statistically significant in Exhibit 4.12) was more evenly distributed across the components of earnings. Throughout the follow-up period this subgroup experienced increases in employment rates, hours worked per week worked, and earnings per hour worked. There was little effect however, on the stability of employment—weeks worked per worker. Overall this pattern is consistent with a program strategy of placing participants as soon as possible in either OJT slots or regular jobs, which should lead to higher employment rates, more full-time work, and work at higher wage rates than the jobs the treatment group would have found in the absence of the program.

The 5.7 percent impact on earnings in the *other services subgroup* was apparently due entirely to a 5.8 percent increase in this group's employment rate. The program impact on weeks worked per worker (3.9 percent) was almost exactly offset by its effect on hours worked per week of employment (-3.7). Earnings per hour employed were not affected by the program. Effects on the proportion employed peaked sharply in the second and third quarters of the follow-up period and then diminished.

This pattern of effects is consistent with a strategy of relatively short-term, low-intensity services aimed at helping individuals find jobs as quickly as possible. The strategy seemed to work in enabling assignees to find jobs sooner than they would have, on average, without access to the program. But it does not appear that the jobs they found were much better in terms of hours per week or rates of pay than the jobs they would otherwise have obtained. Thus, although assignees in the other services subgroup enjoyed substantial earnings gains over the first three quarters as a result of their landing jobs sooner than the control group, by the end of the follow-up period those gains had substantially declined.

The results in Exhibit 4.14 tell a story that is seemingly very coherent and consistent with the goals of each service strategy. Caution must be exercised in their interpretation, however. As noted earlier, the impacts on weeks worked when employed, hours worked per week employed, and the hourly earnings of workers may reflect effects on the composition of the subgroup of assignees who worked, in addition to or instead of effects on the weeks, hours, and hourly earnings of specific individuals. For this reason the estimated impacts on average earnings per hour worked cannot be interpreted as measures of effects on productivity.

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[24] Note that the employment rate in this Exhibit 4.14 (column 2) is expressed as a percentage of the corresponding control group mean. In the preceding exhibit, 4.13 (column 2), the employment rate is expressed as a percentage point difference from the corresponding control group mean.

The analysis of impacts on latent wage rates presented in Appendix G attempts to distinguish between these two different types of effects. None of the estimated impacts on latent wage rates by service strategy subgroup are significantly different from zero. However, as noted earlier, the nonexperimental estimation technique used in Appendix G may not be reliable for reasons discussed in the appendix. There may therefore be no fully satisfactory way to address the compositional issue in the present study.

Impacts on Earnings: Adult Women in Selected Key Subgroups

Up to this point, we have presented impact estimates for all adult women and for those in each service strategy subgroup. To better understand the distribution of program effects, we now examine the impacts of JTPA Title II-A on a number of other subgroups of women of interest to both policymakers and program administrators: those defined in terms of their ethnicity, barriers to employment, work history, welfare history, household composition, family income, and age.[25] This analysis allows us to distinguish the groups for which JTPA was particularly effective, which in turn should help policymakers target future research into the factors that lead to program success. And by identifying any groups for which the program was not working well, the analysis should help policymakers and program administrators target their efforts for improvement.

It is important to note at the outset that any comparison of program impacts across these "key subgroups" must take into account the fact that effects may vary for any of a large number of reasons, reflecting the many dimensions in which subgroups may differ from one another beyond the single, selected characteristic defining them. On average, white women, for example, differ from black women in a variety of ways beyond ethnicity, such as in where they live, their education, and their work experience.

Furthermore, some subgroups that exhibit especially large earnings impacts may have been concentrated in sites with particularly effective programs. Other successful subgroups may have received a particularly effective mix of program services. But the ability of any one of these groups to benefit more from the program may also have been due to factors not directly related to the JTPA program, such as conditions in the local labor market or other personal characteristics of the subgroup members themselves.

To distinguish which of these sets of factors was responsible for differences in program impacts across subgroups, we derived three different impact estimates for each subgroup examined, as shown in the last three column headings of Exhibit 4.15. Column 3 gives the estimated impact on total 18-month earnings for each key subgroup, whereas in column 4 this estimated impact was adjusted for the subgroup's distribution across sites and in column 5, for the distribution of the subgroup across both sites and service strategies. Column 1 gives the size of the sample for each subgroup (with treatment and control groups combined); column 2 shows the mean earnings of the corresponding control group.[26]

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[25] We selected the subgroups examined in this section based on their relevance to policy discussions, before we calculated the estimates. In other words, we did not select them on the basis of the size or significance of the program effects presented here.

It is perhaps simplest to read the exhibit as follows. First, to gauge the variation in impacts across key subgroups, compare the findings for each in column 3. Then, to assess the extent to which the subgroup differences shown stem from differences in the distribution of each subgroup across sites or service strategies, scan across the last two columns. If, for example, the impact estimates in column 3 differ substantially between two subgroups, but those adjusted for the distribution of each of the two subgroups across sites do not, one can conclude that the difference between the column 3 estimates was due to factors associated with the site—either the nature or mix of program services or other characteristics of the local environment.

In almost all cases, the two adjusted estimates for each subgroup are quite similar to the estimate in column 3, in terms of both size and statistical significance.[27] This means that the differences in impacts across the subgroups probably do *not* reflect differences in the distribution of the subgroups across sites or service strategies. They may, of course, reflect differences in the personal attributes of the subgroups' members beyond the characteristic defining the subgroups.

Note that Exhibit 4.15 reports on two tests of statistical significance. The first, indicated by asterisks beside the impact estimate, is the conventional t-test of significance for an individual impact estimate. The results of this test determine our level of confidence that the estimated impact differs from zero. The second test of statistical significance, indicated in the row following each set of subgroups, is a joint F-test of whether the estimated impacts in that set of subgroups differ significantly from one another.

Although a number of the impact estimates for individual subgroups are statistically significant, the F-tests show that in *almost none* of the sets of subgroups do the impact estimates differ significantly from one another at the .10 level. This means that, in general, although we are confident that JTPA had a positive impact on certain subgroups, we cannot be sure whether it had an impact on the other subgroups, and we cannot say with confidence that the impact of the program was greater for the former subgroups than for the latter. The estimates simply are not precise enough to make such distinctions, even in cases of relatively large differences in magnitude between the estimates for any two subgroups in a set. These differences could be due to random sampling error.

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[26] The estimates in columns 4 and 5 adjust the distribution of each subgroup to equal the distribution of adult women overall across sites (column 4) or across sites and service strategies (column 5). For a full description of the methodology for deriving these estimates, see Appendix D.

[27] The only notable exception is the estimated impact on the earnings of Hispanic women.

It is important to bear in mind that the precision of the estimates for any given subgroup depends on its sample size. The estimated impacts for small subgroups are subject to large sampling error. Thus, for small subgroups, only relatively large impacts or large differences in impacts relative to other subgroups can be detected with confidence.

In the discussion of estimated impacts on key subgroups that follows, we focus on the estimates in column 3, because they are experimentally based and thus are the most direct and reliable estimates of the impact. The adjusted estimates in columns 4 and 5, on the other hand, rely on linear adjustments for differences in site and service strategy distributions and therefore may be subject to model specification error.[28]

ETHNIC GROUPS

As shown in the first set of rows in Exhibit 4.15, among white, black, and Hispanic women whites were the only ones to experience a statistically significant earnings impact—with an estimate of $723 over the 18-month follow-up period. Neither the $457 earnings gain of black women nor the $-414 loss of Hispanic women was significantly different from zero.

The estimates for the three ethnic groups are not significantly different from one another at the conventional 90 percent confidence level. They are, however, significantly different at the 80 percent level. Thus, it seems likely that the difference in estimated impacts among these three groups reflects a real difference in program effects, not just sampling error.

Differences in program impacts among women in different ethnic groups could reflect differences in the JTPA services they received. But these differences in impact could also be due to any of a large number of differences among whites, blacks, and Hispanics—for example, differences in personal characteristics, such as educational background, work experience, and family situation or differences in the sites in which they were living, such as unemployment rates, industrial composition, or availability of public transportation.

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[28] All of the subgroup impact estimates in Exhibit 4.15 are based on ordinary least squares regressions on a pooled sample of all adult women, with the treatment indicator interacted with the defining characteristic of the subgroup and (as appropriate) site or site and service strategy. This approach allowed direct calculation of the F-test for differences in impacts among subgroups in each panel of the exhibit. Subgroup impacts were also estimated on samples containing only the subgroup of interest; in general, these estimates differed little from those based on the pooled regressions.

Exhibit 4.15 Impacts on Total 18-Month Earnings: JTPA Assignees, by Selected Key Subgroup

				Impact in $, adjusted for sample distribution across	
Key subgroup, defined by:	*Sample size*[a] *(1)*	*Control mean (2)*	*Impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
Ethnicity					
White, non-Hispanic	3,541	$ 8,007	$ 723***	$ 624**	$ 627**
Black, non-Hispanic	1,981	6,829	457	417	391
Hispanic	744	6,775	-414	81	55
F-test, difference among subgroups			n.s.	n.s.	n.s.
Barriers to employment (in italic)					
Receiving cash welfare	2,446	5,492	387	406	419
No cash welfare[b]	3,500	8,965	697***	664**	647**
F-test, difference between subgroups			n.s.	n.s.	n.s.
No high school diploma or GED certificate	1,731	6,072	416	445	438
High school diploma or GED certificate	4,316	8,064	681***	665***	679***
F-test, difference between subgroups			n.s.	n.s.	n.s.
Worked less than 13 weeks in past 12 months	3,022	5,555	511**	551**	571**
Worked 13 weeks or more in past 12 months	2,622	9,956	668**	600*	561*
F-test, difference between subgroups			n.s.	n.s.	n.s.
Number of barriers					
None of the above	1,361	10,971	909**	814*	737
One of the above	1,655	7,950	802**	775**	819**
Two of the above	1,435	5,756	379	392	372
All three of the above	488	3,703	-213	-86	-129
F-test, difference among subgroups			n.s.	n.s.	n.s.
Work histories					
Never employed	927	4,035	568	623	633
Earned < $4 hourly in last job	1,924	6,901	535*	520*	540*
Earned $4 or > hourly in last job	2,864	8,720	734**	704**	705**
F-test, difference among subgroups			n.s.	n.s.	n.s.
Employed upon application	955	10,497	777	697	666
Not employed upon application	5,470	6,939	509***	522***	535***
F-test, difference between subgroups			n.s.	n.s.	n.s.
AFDC history					
Never AFDC case head	3,104	8,515	673**	675**	693**
AFDC case head less than 2 years	1,441	7,732	437	459	466
AFDC case head 2 years or more	1,773	5,555	570*	542*	525
F-test, difference among subgroups			n.s.	n.s.	n.s.

(Continued)

Exhibit 4.15 *Impacts on Total 18-Month Earnings: JTPA Assignees, by Selected Key Subgroup (continued)*

				Impact, in $, adjusted for sample distribution across:	
Key subgroup, defined by:	*Sample size (1)*	*Control mean (2)*	*Impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
JTPA required for welfare, food stamps, or WIN program [c]					
Yes	762	$ 5,016	$ 529	$ 412	$ 385
No	5,380	7,904	476**	495**	501**
F-test, difference between subgroups			n.s.	n.s.	n.s.
Household composition					
No spouse or own child present	1,270	8,035	533	485	453
Own child under age 4, no spouse, present	1,106	6,718	635	553	550
Own child, none under 4, no spouse, present	1,801	7,661	679*	685*	678*
Spouse present, with or without own child	1,323	7,527	818**	860**	832**
F-test, difference among subgroups			n.s.	n.s.	n.s.
Family income in past 12 months					
$6,000 or less	3,861	6,646	480**	488**	462**
More than $6,000	2,107	9,379	586*	529	572
F-test, difference between subgroups			n.s.	n.s.	n.s.
Living in public housing					
Yes	755	6,370	-466	-420	-436
No	5,582	7,656	675***	670***	674***
F-test, difference between subgroups			*	*	*
Age at random assignment					
22 - 29	2,830	7,265	611*	596*	580*
30 - 44	2,825	7,805	440	451	436
> 44	819	6,897	643	647	780
F-test, difference among subgroups			n.s.	n.s.	n.s.
Recommended for JSA only					
Yes	312	6,815	824	1,136	n/a
No	6,162	7,490	512***	497***	n/a
F-test, difference between subgroups			n.s.	n.s.	n.s.
Full sample	6,474	7,488	539***	539***	539***

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Control means were not regression-adjusted. Sample sizes for mutually exclusive subgroups within a panel do not necessarily sum to the sample size for the target group as a whole, because persons in omitted subgroups or with missing data on the variable used to define the subgroup are excluded.

[a] Treatment and control groups combined.

[b] AFDC, General Assistance, or other welfare except food stamps.

[c] WIN is the federal Work Incentive program.

[*] Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the F-test for the difference in impacts between or among the subgroups in the category is not statistically significant.

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As shown in Exhibit 4.15, the adjustments for differences among ethnic groups in their distribution across sites and across sites and service strategies had little effect on the estimated impacts, with one exception. The adjustments for the distribution of Hispanic women across sites and across sites and service strategies changed the estimated effect from substantially but insignificantly negative to insignificantly positive, eliminating the initial weak evidence that impacts varied with ethnicity. Hispanic women were much more concentrated in a small number of sites than adult women overall. Once these locational differences are removed, any remaining differences in JTPA service mix or in the background and personal characteristics associated with ethnicity were not of themselves sufficient to create detectable variations in impact. Thus, the estimated negative effect on Hispanic women was associated with the geographic distribution of this subgroup. However, given the extreme concentration of Hispanic women in a small number of sites, we cannot reliably distinguish negative effects on Hispanic women as an ethnic group from more general negative site effects on *all* women in one or more of the sites in which the Hispanic women were concentrated.

SUBGROUPS FACING SELECTED BARRIERS TO EMPLOYMENT

Women who apply to JTPA may face a number of barriers to employment. Those receiving welfare face the financial disincentive posed by the loss of welfare and Medicaid benefits should they become employed. Some of these women may also face the barrier of finding adequate child care. Other women applying to JTPA may have limited education or limited recent work experience. To measure the success of JTPA in preparing participants to overcome these barriers, we estimated impacts on women who were receiving cash welfare benefits (AFDC, General Assistance, or other welfare except food stamps), upon their application to the program; women without a high school diploma or GED certificate upon application; and women with less than 13 weeks of employment in the 12 months preceding their application.[29]

The mean earnings of the control groups in column 2 of Exhibit 4.15 indicate that these barriers were indeed serious obstacles to employment for the women in our sample. Control group members in each of the three subgroups facing these barriers to employment earned much less over the 18-month follow-up period ($5,492, $6,072, and $5,555, respectively) than those facing none of these barriers ($10,971).

For comparison purposes the exhibit shows the estimated impacts both on women facing each barrier to employment and on those who were not facing that barrier. The program had no significant effect on the earnings of women who were receiving welfare when they applied to JTPA or on women with less than a high school education, but it did significantly increase the earnings of their counterparts: by $697 for women *not* receiving welfare upon application, and by $681 for women who *had* a high school credential upon application.

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[29] Although the precise measures may differ, the three dimensions of welfare recipiency, limited education, and limited work experience are important and common focal points in much of the literature and agency reporting on JTPA.

Nevertheless, there was no significant difference between the impacts on these subgroups and the impacts on their counterparts who did face the barrier in question (as indicated by the F-test result.) That is, there was no significant difference between the impacts on women receiving welfare upon application and women not receiving it, or between the impacts on women with a high school credential and those without one. Similarly, although we are confident there was a positive effect on the earnings of women with limited recent work experience, we cannot say this effect was greater than that on women with more employment experience in the year preceding their application.

Defining subgroups in terms of the presence or absence of specific barriers to employment does not fully distinguish subgroups in terms of the overall difficulty they faced in becoming employed. For example, the subgroup with limited education includes some women who were also receiving welfare, some with limited recent work experience, and some with both of these other two barriers. We therefore also estimated impacts on women with none, one, two, or all three of these barriers. As shown in the next four rows of the exhibit, this categorization provides clearer distinctions among the subgroups, in terms both of the difficulty of becoming employed and the program impact.

The control group earnings levels indicate that in the absence of the program women facing only one of the three barriers earned more than twice as much ($7,950) as women facing all three barriers ($3,703) and about a third more than those facing two of the three ($5,756). And all three subgroups facing at least one barrier to employment earned much less than the subgroup facing none of the barriers ($10,971).

When categorized by the number of barriers to employment they faced, the only women to experience a statistically significant earnings impact were those facing zero or one barrier. The estimated impact in these subgroups was $909 and $802, respectively, or about 8 percent and 10 percent of the corresponding control group means. About half of the women facing a single barrier had limited recent work experience; the remainder were about evenly divided between those receiving welfare upon application to JTPA and those with limited education (not shown in the exhibit). Thus, these results are consistent with the estimates in the preceding sets of rows in the exhibit showing a significant impact on the earnings of women with limited work experience, but not significant impacts on women in the other two subgroups.

Again, however, the impact estimates for the set of subgroups defined by number of barriers to employment were not significantly different from one another. We can therefore be confident there were positive impacts on those facing one barrier and on those facing no barrier, but we do not know if those impacts were larger than the impacts on the other two subgroups.

SUBGROUPS WITH DIFFERENT WORK HISTORIES

The next panel in the exhibit shows that JTPA significantly increased the earnings of women who had worked before by $535 to $734, including both women who worked for low wages (less than $4 hourly) on their most recent job and higher wage workers. There was no significant effect on the earnings of women who had never worked before, although the estimated impact on their earnings differed little from those on the subgroups who had worked before. Not surprisingly, there was a significant impact on the earnings of women who were not employed upon application to JTPA, who formed the overwhelming majority of the sample (see column 1). The impact on women who were employed was not statistically significant. Within both sets of subgroups, however, the estimated impacts across subgroups were not significantly different from one another.

SUBGROUPS WITH DIFFERENT PUBLIC ASSISTANCE HISTORIES

JTPA had a statistically significant, positive effect on the earnings of women who had never been an AFDC case head. For this subgroup the estimated impact on total 18-month earnings was $673, or about 8 percent of mean earnings in the corresponding control group. Although there have been several experimental studies of employment and training programs for AFDC recipients,[30] to our knowledge this is the first experimentally based estimate of training program impacts on women who are not welfare recipients.

Among women who had been AFDC case heads before applying to JTPA, there was no significant impact on the earnings of those who had been case heads for less than two years, but women who had been case heads for two years or more experienced statistically significant earnings gains of $570 over the 18-month period. There was, however, no statistically significant difference among the estimated impacts for the subgroups defined by AFDC history.

It should be noted that these last two subgroups include all women who had ever been an AFDC case head prior to application. They differ, therefore, from the subgroup receiving AFDC at the time of application, which was discussed above in connection with barriers to employment, although there is a substantial overlap between the two groups.[31]

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[30] See, for example, Enns, Bell, and Flanagan (1987) and Gueron and Pauly (1991).

[31] Among women who had been AFDC case heads for two years or more before applying to JTPA, 79 percent were receiving AFDC at the time of their application. These cases accounted for 57 percent of women in families receiving cash welfare at the time of application. Among women who had been AFDC case heads for less than two years before applying to the program, 61 percent were AFDC case heads when they applied. These cases accounted for 34 percent of women in families receiving cash welfare upon application. (The remaining 9 percent of those in families receiving cash welfare upon application either were receiving cash welfare other than AFDC or were not the AFDC case head.)

There was no significant program effect on the earnings of the small group of women who reported that they were required to apply to JTPA as a condition of receiving welfare or food stamps or as part of the Work Incentive (WIN) program.

SUBGROUPS FROM DIFFERENT TYPES OF HOUSEHOLDS

JTPA had relatively large, significant impacts on the earnings of both women who lived with older children (age 4 and over) but no spouse ($679) and women who lived with their husband with or without a child present ($818). The program did not have a significant effect on the earnings of women who lived without a spouse or child or of those with a young child but no spouse present. The estimated effects for these four subgroups were not, however, significantly different from one another.

The estimated effect on the earnings of women whose family income was $6,000 or less in the year before their application was a statistically significant $480, or about 7 percent of the corresponding control group mean. The effect on women from higher income families ($586) was also significant. The difference in impacts between these two groups was not statistically significant.

The program had no significant impact on the earnings of the relatively small subgroup of women living in public housing upon their application. The estimated impact for this subgroup ($-466) was significantly less than that for all other women ($675), suggesting that women in public housing benefited less from the program.

AGE GROUPS

JTPA had a significant impact on the earnings of only the youngest adult women in the sample, those ages 22 to 29 at random assignment. The estimated effect for this subgroup ($611) did not differ significantly from the impacts for older women, however.

WOMEN RECOMMENDED FOR JOB SEARCH ASSISTANCE ONLY

Finally, to measure the effect on this target group of a low-intensity approach that has frequently been used with welfare recipients, we estimated the impact on the earnings of those women for whom program intake staff recommended job search assistance (JSA) only.[32]

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[32] Estimated impacts for the three major service strategy subgroups were presented earlier and are therefore not repeated here. As with the other subgroups in Exhibit 4.15, we did test for significant differences in impact among the three service strategy subgroups, and estimated impacts adjusted for differences in

These women comprised about a quarter of the other services subgroup.[33] This subgroup was too small to estimate the impact very precisely. Thus, the relatively large estimated impact for this subgroup is not statistically significant, indicating that it may be due to sampling error.

A Summary of Impacts, in the Context of Previous Research

This last section summarizes the impacts of JTPA Title II-A on the earnings, employment, and high school attainment of adult women, with reference to results from previous experimental research where they are comparable to our own. Exhibit 4.16 displays the principal findings in this chapter. We consider our findings for the target group as a whole, summarized first, as benchmarks for drawing conclusions from our more detailed findings for service strategy subgroups and other key subgroups, which we will then examine in turn.

ADULT WOMEN OVERALL

The estimates presented in the first three sections of this chapter document a consistently positive impact on the earnings and employment of adult women. The $539, or 7.2 percent, earnings gain attributable to the program over the follow-up period as a whole was primarily due to program impacts on the percentage of the treatment group who were employed at some time during the period and on earnings per hour worked. There was little or no effect on average weeks worked by those who were employed or on hours worked per week employed. Thus, JTPA appears to have increased the probability that women would be employed, and raised the hourly earnings of those who worked, but it did not lead to steadier employment or more full-time work.

Since not all women in the treatment group actually became enrolled in JTPA, impacts on the average earnings of all JTPA assignees understate the impacts on the earnings of those who were enrolled. Although we cannot estimate impacts on enrollees' earnings with certainty, if we can assume the program had no effect on the earnings on nonenrollees, then the inferred impact on earnings per enrollee was $873 over the entire follow-up period.

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distribution across sites. The estimated impacts for the three service strategy subgroups were not significantly different from one another, and adjustment for differences in distribution among sites had little effect on the impact estimates.

[33] See column 1. As explained in Chapter 2, it was only in the other service subgroup that sample members could be recommended for job search assistance alone.

Exhibit 4.16 *Summary of Estimated JTPA Impacts on Earnings and Employment over the Full Follow-up Period: Adult Female JTPA Assignees in the 18-Month Study Sample, by Service Strategy Subgroup*

Impact per assignee on:	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
Earnings				
In $	$ 539***	$ 398	$ 742**	$ 457
As % of control mean	7.2%	6.2%	8.6%	5.7%
Percentage employed[a]	2.1%**	1.3%	2.4%	4.3%*
Weeks worked				
In weeks	1.1	-0.1	0.7	3.7**
As % of control mean	2.8%	-0.3%	1.6%	9.9%
Hours worked				
In hours	52	-30	97*	79
As % of control mean	3.7%	-2.5%	5.9%	5.7%

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, classroom training subgroup: assignees = 1,916, control group = 931; OJT/JSA subgroup: assignees = 1,538, control group = 749; other services subgroup: assignees = 922, control group = 418. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. At any time during the 18-month follow-up period. The impact is measured in percentage points.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). For the impacts on earnings, weeks worked, and hours worked, the significance level of each estimate expressed "as % of control mean" is the same as that of the corresponding estimate expressed in dollars, in weeks, or in hours.

WOMEN RECOMMENDED FOR EACH SERVICE STRATEGY

The estimated impacts on earnings and employment reflect the increment in employment and training services received by the treatment group as a result of its having access to JTPA. Patterns of service receipt were markedly different across the three service strategy subgroups, but in each case a very high percentage of the women who enrolled in JTPA received one or both of key services provided in the strategy recommended for them. In the classroom training subgroup 88.8 percent of JTPA enrollees received classroom training in occupational skills, basic education, or both. In the OJT/JSA subgroup 87.8 percent of enrollees received on-the-job training, job search assistance, or both. And in the other services subgroup 82.3 percent received job search assistance, miscellaneous services, or both.

Thus, the service recommendations of program intake staff did create three distinct groups in terms of the services their members actually received. The three service strategy subgroups, then, constitute a reasonable test of three distinctly different service strategies.

But the impact of JTPA depends not only on the JTPA services provided to women in the treatment group. It also depends on the services they would have received from other providers had they not had access to JTPA, which we measure by service receipt in the control group.

Direct evidence of the control group's receipt of non-JTPA services was available only for classroom training in occupational skills and basic education. In the classroom training subgroup treatment group members were more likely than control group members to receive classroom training in occupational skills (48.6 percent versus 28.7 percent) and basic education (11.2 percent versus 7.5 percent).

We can also be confident that JTPA substantially increased the receipt of on-the-job training in all three subgroups, since this service is seldom provided by non-JTPA programs. We were unable to measure the extent to which JTPA increased the receipt of job search assistance or miscellaneous services.

While access to JTPA substantially increased the *incidence* of receipt of occupational skills training, basic education, and OJT, the treatment-control differential in hours of occupational skills training *per assignee* for women in the classroom training subgroup was only 110 hours. Similarly, women in the OJT/JSA treatment group received only 104 hours more OJT *per assignee* than their control group counterparts. These relatively small differentials reflect the fact that not all treatment group members received these services and, in the case of occupational skills training, that a nonnegligible proportion of control group members received services.

These service differentials led to significant increases in educational attainment for women in the classroom training and other services subgroups, who were those most likely to receive occupational skills training or basic education. In the classroom training subgroup 29 percent of the high school dropouts in the treatment group achieved a training-related high school diploma or GED certificate, whereas only 11 percent of the corresponding control group did. In the other services subgroup 17 percent of the high school dropouts in the treatment group attained a training-related high school credential, whereas 10 percent of the corresponding control group did. Across all three service strategy subgroups, the program nearly doubled the high school attainment rate among dropouts, from 11 percent to 19 percent.

The estimated effects of JTPA on the earnings and employment of women in the three service strategy subgroups were very consistent with the patterns of effects one would expect, both across the subgroups and over time within each one. For example, women in the *classroom training subgroup* experienced a significant earnings loss in the first quarter after random assignment, reflecting their investment of time in training rather than working. But throughout the rest of the follow-up period they received progressively rising returns on this investment, with impacts on earnings per assignee becoming statistically significant in the fifth quarter and reaching a significant $188 in the sixth quarter. These offsetting losses and gains summed to a statistically insignificant 18-month earnings gain of $398, or 6.2 percent

In contrast, women in the *OJT/JSA subgroup* experienced positive earnings impacts beginning in the first quarter and totaling $742 (8.6 percent) over the follow-up period. The estimated impacts were fairly constant in the $109 to $144 range and were significant in five of the six quarters. This pattern is consistent with an employment strategy that attempts to put women to work immediately, either in an on-the-job training position or in a regular job.

The pattern observed for the *other services subgroup*, on the other hand, was one of relatively quick, but short-lived, effects on earnings. The estimated earnings gain of $220 in the third quarter after random assignment sharply declined and became statistically insignificant in the later quarters. This pattern is consistent with the mix of relatively brief and nonintensive services provided to these women. The total 18-month earnings gain for this subgroup, $457 or 5.7 percent, was not statistically significant.

Although the effects on total 18-month earnings for the classroom training subgroup and the other services subgroup were about the same size, the trends in the effects over time suggest that this overall comparison may be misleading. Because the effects of the other services strategy appear to have peaked and begun to diminish some time before the end of the follow-up period, whereas the effects of classroom training—and OJT/JSA—were growing or stable, it seems likely that the subgroups recommended for these latter two strategies will witness larger long-term effects than the other services subgroup. This issue will be addressed in our forthcoming final report.

The patterns of effects on the components of earnings are also consistent with the divergent nature of the three service strategies. In the classroom training subgroup the earnings gain over the entire follow-up period was largely the result of a positive impact on earnings per hour employed, which offset small decreases in the duration of employment spells and the extent of full-time work. For this subgroup the program had only a small positive effect on the percentage employed. This pattern is consistent with the view that classroom training involves an initial investment of forgone employment and earnings that then leads to higher rates of pay.

In contrast, the overall earnings gain in the OJT/JSA subgroup was more evenly attributable to increases in the employment rate, hours worked per week worked, and hourly earnings when employed. This pattern is consistent with a program strategy of placing participants immediately in OJT slots or regular jobs that offer more full-time work and pay higher wage rates than those the participants would have found in the absence of the program

The impact on earnings in the other services subgroup was apparently due primarily to an increase in this group's employment rate, which peaked in the second and third quarters. This pattern is consistent with a strategy of relatively short-term, low-intensity services aimed at enabling participants to find jobs as quickly as possible. JTPA seems to have been successful in carrying out this strategy for members of this subgroup. But it does not appear that the jobs they found were much better, in terms of their hours per week or rates of pay, than the jobs they would otherwise have gotten.

Despite the apparent coherence and consistency of the story these results tell, it must be remembered that the subgroups differed not only in the nature of the services recommended and received but also in the kinds of women served. For example, intake staff appear to have recommended more job-ready women to the OJT/JSA subgroup. This means that differences in impacts across the subgroups cannot be attributed solely to differences in services. Thus, while we can identify those service strategies that were working, or not working, for the groups of women they actually served, we cannot say whether the impacts for any subgroup of women could be improved by substituting a different mix of services for the one the subgroup members actually received.

Furthermore, the costs of these three service strategies were likely to be quite different. Differences in impacts therefore may not accurately reflect differences in cost-effectiveness. Our final report will present a benefit-cost analysis that takes these cost differences into account.

To provide a broader context within which to assess the impacts reported here, Exhibit 4.17 shows the impacts estimated in previous experimental evaluations of employment and training programs serving women.[34] In all cases these programs served only AFDC recipients or applicants; we know of no experimental studies of programs serving other women. Thus, the participant populations in these studies are not fully comparable with the sample in the National JTPA Study. Moreover, unlike in JTPA, participation in many of these previous programs was mandatory, with potential sanctions for nonparticipation.

The services provided varied substantially across these programs. In the discussion here we will therefore compare the impacts of each program to the impacts of the JTPA service strategy that involved the most comparable services. Note that the impact estimates shown have been calculated as *average quarterly* impacts during each of the first three years following random assignment (columns 3 through 5). The year 1 impact estimates shown for JTPA are averages of the estimates for the first four quarters after random assignment; the year 2 estimates are averages of the fifth and sixth quarter estimates only.

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[34] The studies included are only those with final impact results available. The estimates from the previous studies have been converted to July 1989 dollar values.

Exhibit 4.17 Estimated Impacts on the Average Quarterly Earnings of Adult Women: The JTPA 18-Month Impact Analysis and Previous Experimental Studies

Program (Year evaluation began)	Treatment group (1)	Program services (2)	Quarterly $ impact per treatment group member (% impact in parentheses) Year 1 (3)	Year 2[a] (4)	Year 3 (5)
National JTPA Study (1987)	Low-income women, 34% receiving AFDC, 74% with child of any age; participation voluntary	Classroom training strategy: occupational skills training, basic education[b]	$16 (2%)	$ 166** (13%)	--
		OJT/JSA strategy: on-the-job training, job search assistance	$ 116*** (8%)	$ 140** (9%)	--
		Other services strategy: job search assistance, miscellaneous[c]	$103 (8%)	$22 (1%)	
		All three strategies	$71** (6%)	$129*** (9%)	--
Work-welfare (WIN) studies:					
Low-intensity programs (1978-1985)[d]	AFDC applicants and recipients;[e] participation mandatory	Job search assistance, unpaid work experience, occupational skills training, basic education[b]	$ 3 to 138*** (1%, 23%)	$ 65 to 132**[f] (23%, 20%)	$ 76* to 126**[f] (11%, 18%)
Moderate-intensity programs[g] (1982-1985)	AFDC applicants and recipients with youngest child age 6 or older; participation mandatory	Job search assistance, unpaid work experience, occupational skills training, basic education[b]	$ 43 to 100*** (10%, 21%)	$118*** to 180*** (17%, 29%)	$145***[h] (17%)
OJT programs[i] (1983-1984)	AFDC recipients with child of any age; partipation voluntary	Job-readiness training, unpaid work experience, on-the-job training	$31[j] (8%)	$165* to 249** (14%, 38%)	265*[j] (34%)
Other work-welfare studies					
AFDC Homemaker-Home Health Aide Demonstrations[k] (1983)	AFDC recipients with child of any age; partipation voluntary[l]	Occupational skills training and practicum in home care, up to 12 months subsidized employment	$ 113 to 1,058*** (15%, 271%)	$147** to 669*** (16%, 76%)	$ 39 to 573*** (13%, 78%)
National Supported Work Demonstration[m] (1976)	AFDC recipients for 30 of past 36 months, with child of any age; participation voluntary	Structured, paid work experience for up to 12 months	$ 1,847*** (--)[n]	$ 395*** (36%)	310*** (23%)

Sources: Reviewed in Gueron and Pauly (1991)

Notes: The reported estimates have been converted to July 1989 dollars. The "% impact" is the "$ impact" expressed as a percentage of the control group mean.

(continued)

The first panel of the exhibit summarizes the impact estimates presented earlier in this chapter for adult women in each of the three JTPA service strategy subgroups and for the target group as a whole. The next panel presents results from eleven studies of state Work Incentive (WIN) programs and WIN demonstration work-welfare programs conducted in the late 1970s and early 1980s. The first two sets of work-welfare programs, those offering low- and moderate-intensity services, were mandatory for certain groups of AFDC applicants and recipients. In the first set seven low-intensity programs offered primarily group and individual job search assistance and unpaid work experience. The two moderate-intensity programs in the second set offered these services as well as occupational skills training and basic education for up to a quarter of the participants. And the two demonstrations in the third set emphasized on-the-job training for voluntary participants already receiving AFDC.

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[a] The estimates from the National JTPA study (first panel) in this column are for the first two quarters only.

[b] "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

[c] "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

[d] Included the following programs (net cost per treatment group member, in nominal dollars except where so noted, in parentheses): Arkansas WORK Program ($118), Cook County (Illinois) WIN Demonstration ($157), Louisville WIN Laboratory Experiments ($136, individual job search assistance, $230, group job search assistance, converted to 1985 dollars), San Diego Employment Preparation Program/Experimental Work Experience Program ("San Diego I") ($636), Virginia Employment Services Program ($430), and West Virginia Community Work Experience Program ($260).

[e] Targeted to AFDC applicants and recipients with youngest child age 6 or older, except in Arkansas (child 3 or under); Louisville (child any age); San Diego I (AFDC applicants only, excluding those who were refugees, employed, or monolingual in a language other than English or Spanish); Virginia (excluded previous program participants and current full-time participants in other education or training services); and West Virginia (excluded those employed or in full-time education or training).

[f] Impact estimates for years 2 and 3 available only for Arkansas, Louisville (individual job search assistance only), and Virginia

[g] Included the following programs (net cost in nominal dollars per treatment group member in parentheses): San Diego Saturation Work Initiative Model (SWIM) ($919) and Baltimore Options Program ($953).

[h] Impact estimates for year 3 available only for Baltimore.

[i] Included the following programs (net cost in nominal dollars per treatment group member, including wage payments for subsidized employment, in parentheses): Maine On-the-Job Training Program ($2,019) and New Jersey On-the-Job Training Program ($787).

[j] Impact estimates for years 1 and 3 available only for Maine.

[k] Included demonstrations in the following states (net cost in nominal dollars per treatment group member, including wage payments for subsidized employment, in parentheses): Arkansas ($5,367), Kentucky ($6,955), New Jersey ($7,067), New York ($3,651), Ohio ($5,977), South Carolina ($7,907), and Texas ($6,267).

[l] In South Carolina targeted to AFDC recipients with youngest child age 11 or older.

[m] Included demonstrations in seven sites, with a net cost per treatment group member of $8,093 (1976 dollars), or $10,231 and thereby receiving heavily subsidized earnings during that year.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

The low-intensity programs are probably most comparable to JTPA's other services strategy, while the OJT demonstrations are most comparable to the OJT/JSA strategy. The moderate-intensity programs probably fall between the JTPA classroom training subgroup and the JTPA other services subgroup in terms of the intensity of services.

The impact of the JTPA *other services strategy* appears quite comparable to that of the low-intensity work-welfare programs in year 1 but not in year 2. In both dollar and percentage terms the work-welfare programs had much larger impacts than JTPA in the second follow-up year, effects that continued throughout the three-year follow-up periods for the studies. In contrast, as shown earlier in this chapter (Exhibit 4.12), earnings gains from the JTPA other services strategy had ended by the fourth quarter after random assignment.

The impacts of the OJT work-welfare programs also appear to have been larger after year 1 than that of JTPA's *OJT/JSA strategy*. In dollar terms this component of JTPA had a much larger impact than the two WIN demonstrations in year 1 and a smaller impact in year 2. In percentage terms, on the other hand, JTPA and the work-welfare OJT programs for which impact estimates are available had the same impact in year 1, but work-welfare had much larger impacts than JTPA in year 2. Overall, the order of magnitude of the impacts appears to have been similar for the three programs.

None of the previous employment and training programs for women that have been rigorously evaluated with experimental designs offered a service mix that is closely comparable to the JTPA *classroom training strategy*. As noted above, the two moderate-intensity work-welfare demonstrations most closely resembled the JTPA classroom training service strategy, but the services they offered were somewhat less intensive. The estimated dollar impact of the JTPA classroom training strategy was quite comparable to those of the moderate-intensity work-welfare programs in the second year, but the JTPA impact was much smaller in percentage terms, reflecting the lower earnings levels of control group members in the work-welfare programs.

The AFDC Homemaker–Home Health Aide Demonstrations, shown in the bottom panel of Exhibit 4.17, offered four to eight weeks of classroom training but also provided up to a year of subsidized employment, including substantial on-the-job training. Similarly, the National Supported Work Demonstration offered up to 12 months of subsidized employment. Thus, it should not be surprising that both of these programs had substantially larger estimated impacts than any of the JTPA service strategies. It should also be noted that the estimated impacts shown for these demonstrations include substantial amounts of wage subsidies paid by the demonstrations in the first two years.

In general, then, to the extent that comparable impact estimates are available from rigorously evaluated programs for women, they tend to be similar to or slightly more favorable than those found here for JTPA. These comparisons must be viewed with caution, however, because both the services and the participants in these earlier programs differed in important ways from those in JTPA. In particular, all of the other programs served only AFDC applicants or recipients, whereas only a third of the adult women in our 18-month study sample were receiving AFDC upon application. Moreover, participation was mandatory for AFDC recipients in some of these programs. There were also differences in the extent to which treatment group members in the various studies actually received any program services, with service receipt rates ranging from a low of 38 percent in one of the work-welfare studies to 97 percent in Supported Work. (Recall that 65 percent of the adult female treatment group members in our 18-month study sample were enrolled in JTPA services.) Finally, a full comparison with the JTPA sample must await the longer term follow-up and benefit-cost analysis we will present in our final report.

Women in Selected Key Subgroups

In addition to presenting estimated impacts for the three service strategy subgroups, this chapter has also provided estimated impacts for a variety of other subgroups of women of interest to policymakers and program planners. Those results showed significant positive impacts on total 18-month earnings for a number of subgroups of women. In most cases, however, the differences in estimated impacts among subgroups were small, and in only two cases did these differences even approach conventional levels of statistical significance. Those two cases were Hispanic women and the very small subgroup of women who lived in public housing. Neither of those subgroups experienced significant earnings gains as a result of the program, and the estimated impacts on their earnings were substantially and significantly less than the estimated impacts for other women (namely, white women and women not living in public housing, respectively).

In all other cases, while we can be more confident that the program had a positive impact on those subgroups with statistically significant estimated impacts, we cannot say with confidence that those impacts were *larger* than the impacts on the subgroups whose impact estimates were not significant.

We also tested to see whether variations in impacts across subgroups were due to differences in the distributions of subgroups across sites or sites and service strategies. The estimated impacts were largely invariant with adjustments for these distributional differences among subgroups. We therefore conclude that variations in impacts across subgroups reflect primarily differences in the characteristics of the women themselves (and random sampling error), not differences in where they were living or the services they received. The one exception to this general statement was the estimated impact for Hispanic women. This difference in effects appears to be associated with the geographic distribution of Hispanic women. But because of the extreme concentration of Hispanic women in only a few sites, we cannot reliably distinguish whether the negative impact on this subgroup was associated with ethnicity *per se* or with a more general negative effect on *all* women in one or more of the sites in which the Hispanic women were concentrated

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Adult Men: JTPA Impacts at 18 Months

THIS chapter presents estimates of the impact of JTPA Title II-A programs at the 16 study sites on the earnings, employment, and high school attainment of adult men over the 18 months following their random assignment. These findings offer the first reliable information about the effectiveness of Title II-A programs for men and considerably expand existing knowledge about employment and training programs serving economically disadvantaged men in general.

The 5,626 men in the 18-month study sample were ages 22 or older at their random assignment; and like the adult women in the sample, they had an average age of 33, with 88 percent between ages 22 and 44 and less than 5 percent age 55 or older.[1] The men also resembled the women in ethnicity, with 57 percent white, 29 percent black, and 10 percent Hispanic. But as we might expect, the men had, on average, higher wage rates in their most recent job, as well as more extensive employment experience. Over 90 percent of the men had held a job in the past. Nevertheless, only 13 percent were employed upon application to JTPA. Like the women, over two-thirds of the men had a high school credential. In contrast, only 6 percent were receiving Aid to Families with Dependent Children (AFDC) when they applied to the program, while 27 percent were receiving food stamps—rates far below those of the women.[2]

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[1] Among all the men in the 18-month study sample, 4,519, or 80.3 percent, responded to the First Follow-up Survey. The information on baseline characteristics presented here is based on Background Information Form responses from all men in the 18-month study sample.

[2] See Exhibit 3.13 in Chapter 3 for more detail.

The structure of this chapter repeats that of the preceding one. We first present estimates of JTPA impacts on adult men overall, in three sections examining, in turn, impacts on earnings, on employment, and on the components of earnings. Unlike the preceding chapter, the impact estimates throughout are based exclusively on First Follow-up Survey data, because tests of survey nonresponse bias indicated negligible nonresponse bias in the data for adult men.

In the fourth section of the chapter, we turn to estimated impacts on the subgroups of adult men recommended for each of the three service strategies defined in Chapter 2: classroom training, OJT/JSA, and other services. The fifth section gives estimated impacts for selected key subgroups of men defined in terms of individual characteristics, such as barriers to employment, that may have affected their ability to benefit from JTPA. Finally, in the chapter summary we review the findings and place them in context with estimates from previous experimental studies of employment and training programs serving adult men.

Impacts on Earnings: Adult Men Overall

This section presents estimates of JTPA Title II-A impacts on the earnings of adult men. We begin by simply contrasting the monthly earnings trends of the treatment and control groups. We then describe the pattern of treatment group enrollment in Title II-A over the 18-month follow-up period to draw a distinction between the in-program and post-program periods for the group as a whole. The section next presents numerical estimates of program impacts on earnings per JTPA assignee and per enrollee and ends by discussing impacts on the distribution of earnings.

MONTHLY EARNINGS TRENDS: TREATMENT GROUP AND CONTROL GROUP

Exhibit 5.1 displays the trends in the average monthly earnings of treatment group and control group members during the 18 months following their random assignment.[3] The exhibit shows that average earnings were virtually the same for the two groups during the first 3 of the 18 follow-up months. After that point, treatment group members, on average, consistently earned more than their control group counterparts.

The estimates underlying the earnings curve for the treatment group represent an *outcome* of JTPA—what the treatment group earned after its members gained access to JTPA: from

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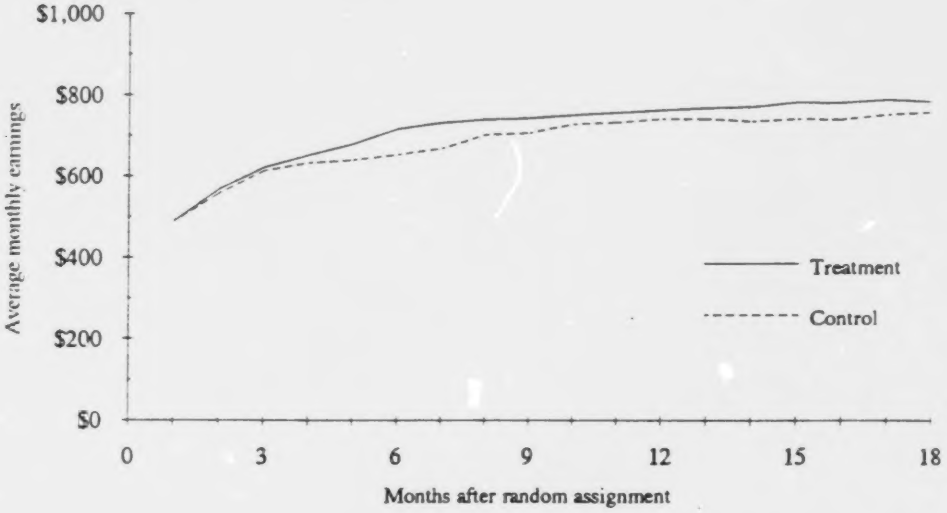
[3] Throughout this chapter earnings and impact estimates are expressed in nominal dollars. The follow-up period varied across individuals, beginning as early as November 1987 and ending as late as December 1990.

Exhibit 5.1 Average Monthly Earnings: Treatment Group and Control Group



Source: Estimates based on First Follow-up Survey responses.
Notes: Sample size, treatment group = 2,980, control group = 1,439. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

$493 in the first follow-up month to $801 in the eighteenth.[4] But this information does not tell us what the treatment group would have earned without access to JTPA, which is what we must know to calculate the *impact* estimates we will present shortly.

It is the estimates underlying the control group curve that provide this information. Average earnings in the control group ranged from $501 in the first follow-up month to $774 in the eighteenth.[5] These point estimates lend two critical pieces of information to the analysis. First, they represent our estimate of what the treatment group would have earned, on average, without access to JTPA—the basis for the treatment-control group comparisons of the experimental impact analysis. And second, they also demonstrate the importance of making these comparisons in evaluating employment and training programs, since they show that, even without access to JTPA, control group members increased their average monthly earnings substantially over the 18-month follow-up period.

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[4] We used ordinary least squares regression procedures to increase the statistical precision of these estimates, as described in Appendix D.

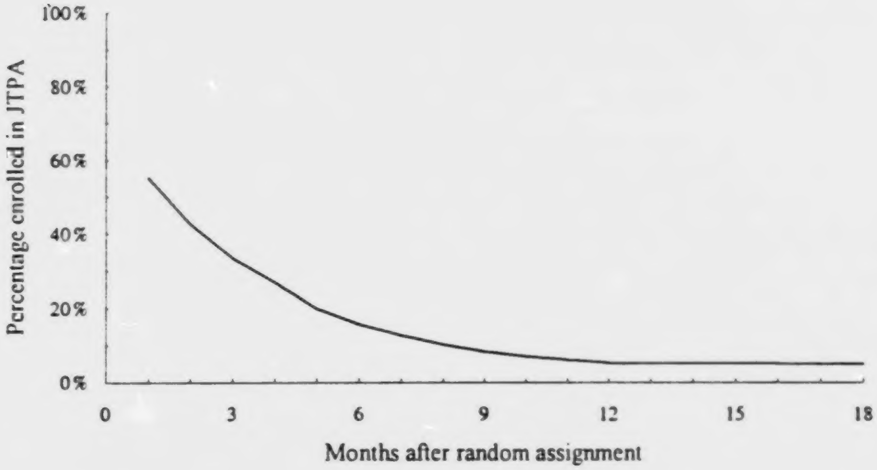
[5] The earnings estimates shown in Exhibit 5.1 and subsequent exhibits include wages paid to JTPA participants in on-the-job training positions. During the 18-month follow-up period the program reimbursed employers a total of about $700 per adult male *OJT participant*. Among all adult men in the sample, OJT reimbursements totaled about $100 per *treatment group member* over the 18-month follow-up period.

Enrollment Patterns over Time: Treatment Group

As explained in the previous chapter, any difference in earnings between the treatment group and the control group may be partly affected by the treatment group's participation in the program. In the early months of the follow-up period, enrollment in classroom instruction may have actually delayed employment for some treatment group members, whereas for others enrollment in on-the-job training may have led to faster job placements and thus higher wages than their control group counterparts during those months. To interpret properly the pattern of treatment-control group differences, we must therefore distinguish between when, during the follow-up period, most treatment group members were enrolled in JTPA and when most of them were no longer enrolled.

Exhibit 5.2 allows us to make this distinction between the *in-program period* and the *post-program period*.[6] Among adult men, enrollment rates of the treatment group dropped

Exhibit 5.2 Percentage Enrolled in JTPA Monthly: Treatment Group



Source: Unadjusted frequencies based on enrollment and tracking data from the 16 service delivery areas (SDAs).
Note: Sample size, treatment group = 3,759.

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[6] Exhibit 5.2 is based on data from SDA records that somewhat overstate the number of persons still in the program at any given time because the data are missing some termination dates. Thus, this graph serves as an upper bound on the percentage of the treatment group still in the program in any given month. The extent to which the graph overstates the actual enrollment rates is higher in the later months.

from 55 percent in the first follow-up month to only 5 percent in the eighteenth.[7] We are using the first month in which less than 15 percent of the treatment group was enrolled as the beginning of the post-program period, which was in month 7 for adult men. Thus, by referring back to Exhibit 5.1, we can see that the positive treatment-control group difference in earnings began before most treatment group members left the program (month 3) and persisted well after most of those who were enrolled had left the program (months 7 through 18).

IMPACTS ON EARNINGS: JTPA ASSIGNEES AND ENROLLEES

Exhibit 5.3 displays estimates of the average program impacts on the earnings of adult men in the sample during each of the first six *quarters* after random assignment and over the 18-month follow-up period as a whole. As was the case for adult women in Chapter 4, it is important to note that these estimates of program impacts for men represent the *incremental* impact of JTPA, over and above the effects of other employment and training services that were available to control group members. Because not all treatment group members received employment and training services from JTPA and because some control group members received employment and training services from non-JTPA providers, the service differential (increment) between treatment and control group members was limited. To help interpret the magnitudes of these impact estimates, our final report will compare them to the corresponding differentials in the costs of the employment and training services received by treatment and control group members.

Column 1 of the exhibit shows the estimated average quarterly earnings of the *control group*, which rose from $1,659 in the first follow-up quarter to $2,242 in the sixth, for an average total earnings over all quarters of $12,306. Column 2 presents dollar estimates of program *impacts* on earnings *per JTPA assignee* (treatment group member), obtained by taking the difference between the average earnings of treatment group members and the average earnings of control group members.[8] Column 3 expresses these dollar estimates as a percentage of the control group mean for the corresponding time period.[9] These estimated impacts reflect how much more the treatment group earned as a result of its members having access to the program.

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[7] Although 61 percent of the adult male treatment group was enrolled in JTPA Title II-A at some point during the follow-up period, only 55 percent was enrolled in the first follow-up month, because some treatment group members were enrolled later.

[8] To increase the statistical precision of these estimates, we used ordinary least squares regressions. This reduced the standard errors of the impact estimates but did not appreciably affect the point estimates, because the average values of the independent variables (mainly the baseline characteristics of the treatment and control groups) were virtually the same for the two groups. See Appendix D for a full description of these procedures.

[9] For simplicity's sake, we do not report significance levels for the percentage impacts because they are the same as those in the corresponding rows of column 2.

Exhibit 5.3 Impacts on Earnings: JTPA Assignees and Enrollees

Period	Control mean (1)	Impact per assignee		Inferred impact per enrollee, in $ (4)
		In $ (2)	As % of (1) (3)	
Quarter 1	$ 1,659	$ 17	1.1%	$ 30
2	1,925	121*	6.3	205
3	2,073	138**	6.7	235
4	2,196	68	3.1	115
5	2,212	103	4.7	175
6	2,242	102	4.6	174
All quarters	12,306	550	4.5	935

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 2,980; control group = 1,439. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

The estimated impacts varied from a low of $17 in the first quarter to a high of $138 in the third. In percentage terms this range was from 1.1 percent to 6.7 percent. For the follow-up period as a whole, the estimated impact on earnings per assignee was $550, or 4.5 percent.

As shown, the only impact estimates that were statistically significant at conventional levels were those for the second and third follow-up quarters. Hence, it is only for those quarters that we can be confident that the estimate represents a true impact, instead of a chance difference resulting from random sampling error. Nevertheless, two of the other quarterly earnings estimates in Exhibit 5.3 were statistically significant at very near conventional levels, as was that for the follow-up period as a whole.[10] Since the estimated impacts on *employment* for the target group as a whole, presented later in this chapter, were highly significant, we will at that point offer further interpretation of the earnings estimates presented here.

Impact estimates per JTPA assignee represent the effect of having access to Title II-A. But not all treatment group members actually enrolled in the program, and thus we calculated separate estimates of *impacts per JTPA enrollee*. As discussed in Chapter 2, however, we cannot provide a fully reliable estimate of the impact per JTPA enrollee. We can, however, infer the impact per enrollee by adjusting the estimated impact per assignee to account for the facts that 38.7 percent of all adult male assignees did not become enrolled in JTPA and 2.2 percent of adult male control group members did enroll (despite the experiment's embargo on their participation).[11] As in the preceding chapter, we view these inferred impacts *per*

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[10] Estimates for the fifth and sixth quarters, and for the follow-up period as a whole, were statistically significant at the .15 level (two-tailed test).

[11] Appendix D details the procedures used for these adjustments.

enrollee as estimates of the upper bound on the average program impact on enrollees, and we view the estimated impacts *per assignee* as estimates of the lower bound on the average impact on enrollees.[12]

Column 4 in Exhibit 5.3 displays our inferred estimates of impacts on earnings per enrollee.[13] For the third quarter after random assignment (the quarter that exhibited the largest impact per assignee), the estimated impact per enrollee was $235. Thus, for the third quarter our best estimate of the average impact on enrollees is in the range between $138 (the estimated impact per assignee) and $235 (the inferred impact per enrollee).

Recall from Chapter 4 that these inferences are based on the assumption that JTPA had no impact on those assignees who did not enroll in the program. If these *nonenrollees* received little or no JTPA service, and thus experienced a negligible JTPA impact, the true impact on enrollees was probably closer to $235 in the third quarter.[14] But if nonenrollees did receive substantial JTPA services, and thus experienced impacts similar to those that the enrollees experienced, the true impact on enrollees was probably closer to $138 in that quarter. Without complete information on the JTPA services received by nonenrolled treatment group members, we cannot determine where in this range the answer lies. To reflect this uncertainty, we did not attempt to calculate the statistical significance of the inferred impacts per enrollee, and in the remainder of this chapter we focus primarily on impacts per assignee.

IMPACTS ON THE DISTRIBUTION OF EARNINGS: JTPA ASSIGNEES

The preceding findings represent the *average* program impacts per assignee or per enrollee. Among individual men in the sample some may have experienced larger impacts, while others experienced smaller, negligible, or even negative impacts. To better understand how JTPA works, we would also like to know what this distribution of impacts looks like. For example, does the $138 average impact per assignee in the third quarter represent a large positive impact on a few sample members or a modest positive impact on many?

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[12] It is possible that the program impact on nonenrolled treatment group members was the opposite of any impact on the enrollees. If this were so, which seems unlikely, the estimated impacts per enrollee would understate the magnitude of the average impact on enrollees.

[13] As explained in Appendix D, the adjustment factor used to derive impacts per enrollee from impacts per assignee is $1/(r-c)$, where r is the enrollment rate (the proportion of treatment group members who were enrolled in JTPA) and c is the crossover rate (the proportion of control group members who were enrolled in JTPA). Since these two rates are fixed for any given group or subgroup, the ratio of impacts per enrollee to impacts per assignee is also fixed for any given group or subgroup. Thus, for example, for the adult male target group, the impact per enrollee is 1.71 times the impact per assignee for all outcomes in all time periods.

[14] Appendix F explores the evidence of JTPA services received by treatment group nonenrollees.

As explained in Chapter 4, we cannot answer this question because we cannot directly observe the distribution of program impacts on the earnings of individual treatment group members. We can, however, observe average program impacts on the *earnings distribution* of the treatment group. Here the question is how JTPA changed the shape of the earnings distribution. For example, did the program reduce the percentage of treatment group members with low earnings, or did it increase the percentage with higher earnings?

Exhibit 5.4 addresses these questions by comparing the distributions of total 18-month earnings for JTPA assignees (the treatment group) in column 1 and for the control group in column 2. As in Chapter 4, the categories of earnings shown on the left side of the exhibit are defined so as to divide the earnings distribution of the control group into those with zero earnings and quartiles of those with positive earnings.[15] Column 3 shows the difference between the two groups for each of the five earnings categories. These differences represent the impacts of JTPA on the earnings distribution.

Exhibit 5.4 Impacts on the Distribution of Total 18-Month Earnings: JTPA Assignees

18-month earnings	*Assignees (1)*	*Control group (2)*	*Difference, in % points (3)*
$0	13.2%	15.6%	-2.4%**
$1 - $6,400	21.0	21.1	0.0
$6,401 - $13,300	22.1	21.8	0.4
$13,301 - $20,700	22.0	20.5	1.4
> $20,700	21.8	21.1	0.7
Chi-squared test of impact on entire distribution			not significant

Source: Unadjusted frequencies based on First Follow-up Survey responses.

Notes: Sample size, assignees = 2,980; control group = 1,439. For the estimation procedure, see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (chi-squared test or two-tailed t-test).

The zero earnings category exhibits the largest treatment-control group difference, and the only one that is statistically significant. This finding indicates that JTPA reduced the percentage of assignees with zero earnings by -2.4 percentage points. In other words, the program increased the percentage of assignees who became employed, which is consistent with the estimated impacts on employment, that we will turn to next. Because the treatment group and control group percentages were quite similar for the other four categories, our test of the *overall* impact on the earnings distribution was not statistically significant.

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[15] The distribution of control group members for those with positive earnings does not equal exact quartiles because the earnings categories used to define this distribution were specified to the nearest $100.

Impacts on Employment: Adult Men Overall

This section examines the impacts of JTPA Title II-A on the employment of adult men. We report estimated impacts using three measures of employment—the *percentage employed*, the average number of *weeks worked*, and the average number of *hours worked*—for each follow-up quarter and for the 18-month period as a whole. All three measures include zeros for sample members who were not employed.

Exhibit 5.5 displays the three measures in separate panels. Note first that the top panel indicates that 59.4 percent of the control group was employed at some time during the first quarter after random assignment. The employment rate of the control group grew continually over the remaining follow-up quarters, reaching 69.2 percent in the last one. During the 18 months as a whole, 83.6 percent of the control group was employed at some time.[16]

Nevertheless, the estimated impacts per assignee on the percentage employed were statistically significant for the second, third, and fourth quarters and for the follow-up period as a whole, ranging in these quarters from an estimated 3.4 to 5.2 percentage points (column 2). The inferred impacts per enrollee ranged from an estimated 5.7 to 8.9 percentage points in those quarters (column 4). Overall, the program increased the proportion of the treatment group ever employed during the 18-month period by an estimated 2.8 percentage points for assignees (which was statistically significant) and by an estimated 4.8 percentage points for enrollees. Note, however, that the impact on employment rates appeared to decline toward the end of the follow-up period.

The middle panel in the exhibit shows estimates of program impacts on the number of weeks worked by sample members. This measure is an average for all adult men in the sample, and thus it includes zeros for men who were not employed. The average number of weeks worked by control group members rose continually over the period, which again indicates that the labor market prospects of treatment group members would have improved to some extent even without access to JTPA. But JTPA had a statistically significant impact on weeks worked in every follow-up quarter except the first, when many treatment group members were still in the program. The estimated impacts per assignee ranged from 0.3 to 0.5 weeks, or 4.4 percent to 6.7 percent. The inferred impacts were larger by a factor of 1.71 for enrollees.[17] Over all quarters the program increased the number of weeks worked by assignees by 2.2 weeks, or 4.8 percent, which was statistically significant.

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[16] Because the outcome shown in the top panel of Exhibit 5.5 is the percentage of the sample *ever* employed in the relevant time period (quarter or 18-month follow-up period), quarterly proportions do not sum or average to the proportion for the follow-up period as a whole.

[17] The ratio of impacts per enrollee to impacts per assignee may differ somewhat from 1.71 because of rounding.

Exhibit 5.5 Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: JTPA Assignees and Enrollees

		Impact per assignee		
Period	*Control mean* *(1)*	*In % pts., weeks, or hours* *(2)*	*As % of (1)* *(3)*	*Inferred impact per enrollee, in % pts., weeks, or hours* *(4)*
		Percentage employed		
Quarter 1	59.4%	1.1%	1.9%	1.9%
2	63.2	4.1***	6.4	6.9
3	65.2	5.2***	8.0	8.9
4	67.2	3.4**	5.0	5.7
5	69.0	1.6	2.4	2.8
6	69.2	2.0	2.8	3.3
Anytime during quarters 1 - 6	83.6	2.8**	3.4	4.8
		Weeks worked		
Quarter 1	6.5	0.0	-0.5%	-0.1
2	7.2	0.4**	5.3	0.6
3	7.5	0.5***	6.7	0.9
4	7.8	0.3*	4.4	0.6
5	7.9	0.4**	5.6	0.7
6	8.4	0.5**	6.3	0.9
All quarters	45.3	2.2**	4.8	3.7
		Hours worked		
Quarter 1	267	-3	-0.9%	-4
2	302	14	4.6	24
3	315	21**	6.6	36
4	328	10	3.2	18
5	326	21**	6.4	35
6	328	20**	6.2	35
All quarters	1,865	84**	4.5	143

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 2,980; control group = 1,439. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

The bottom panel in Exhibit 5.5 shows the estimated impacts on the average number of hours worked, which again includes zeros for men who were not employed. The average for control group members rose until the fourth quarter but then remained about the same. Average hours worked among assignees, on the other hand, rose by even more during the period, and program impacts were statistically significant in three of the last four quarters. Over the full follow-up period the estimated impact per assignee was also statistically significant at an additional 84 hours, while the inferred impact per enrollee was 143 hours.

Thus, the estimated impacts of JTPA on the overall employment of men were statistically significant in most quarters and for the follow-up period as a whole. If, as indicated in the next section, the program did not reduce the average hourly earnings of those men who were employed, this increase in employment implies a corresponding increase in earnings. Together with the fact that the estimated impacts on earnings presented in the previous section were statistically significant at close to conventional levels, this evidence suggests that the program did increase average earnings for men.

Impacts on the Components of Earnings: Adult Men Overall

In this section we attempt to distinguish between the effects on the average number of weeks and hours worked per assignee that simply reflect higher employment rates among all assignees and those that reflect additional *weeks* and *hours* worked among only those men who were employed. As in the previous chapter, we do this by shifting from average weeks and hours worked by *all* assignees (including those who did not work) to average weeks and hours per week worked by only those *assignees who did work*. This analysis is based on the fact that average earnings per assignee can be decomposed as follows:

$$\frac{\text{earnings}}{\text{assignee}} = \frac{\text{workers}}{\text{assignee}} \times \frac{\text{weeks}}{\text{worker}} \times \frac{\text{hours}}{\text{week}} \times \frac{\text{earnings}}{\text{hour}}$$

Each of the four *components of earnings* in this relationship reflects a different aspect of labor market success. *Workers per assignee* reflects the ability of assignees to find jobs—the "pure" employment effect. *Weeks worked per worker* reflects both how quickly assignees found jobs and how long they held them, or the stability of their employment. *Hours worked per week worked* reflects the mix of workers' part-time, full-time, and overtime work, that is, whether they were more likely to find a full-time job. And *earnings per hour worked* reflects what workers were paid for the time they worked. Appendix D explains how we estimated program impacts on each of these earnings components, and Exhibit 5.6 presents our findings.

Exhibit 5.6 Percentage Impacts on Earnings and Its Components: JTPA Assignees

Period	*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
Quarter 1	1.1%	1.9%	-2.3%	-0.4%	2.0%
2	6.3	6.4	-1.1	-0.6	1.6
3	6.7	8.0	-1.2	0.0	0.0
4	3.1	5.0	-0.6	-1.2	-0.1
5	4.7	2.4	3.1	0.8	-1.6
6	4.6	2.8	3.4	-0.1	-1.5
All quarters	4.5	3.4	1.4	-0.3	0.0

Source: Estimates based on First Follow-up Survey responses.
Notes: Sample size, assignees = 2,980; control group = 1,439. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.
Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is displayed as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

Column 1 in the exhibit repeats the estimates of *percentage impacts on earnings* per JTPA assignee given earlier in Exhibit 5.3. The remaining four columns present estimates of the percentage impact on each earnings component. Because the percentage impacts on the four components in each row sum to approximately equal the percentage impact on earnings in the same row, they indicate the relative contribution that each component made to the impact on earnings. No significance levels are shown in Exhibit 5.6. The significance levels of the *impacts on total earnings* (column 1) and *workers per assignee* (column 2) are the same as those in Exhibits 5.3 (for earnings) and 5.5 (for percentage employed). Because the last three columns were estimated indirectly, no tests of significance were calculated for the estimated impacts on these outcomes.[18]

The results indicate that the positive program impact on the earnings of men reflects an increase in the amount of time they worked, not an increase in the average amount they were paid for the time they worked. This conclusion holds for all six follow-up quarters, but it is most obvious in the row for the full follow-up period, where the 4.5 percent impact on total earnings is manifested almost exclusively in the 3.4 percent increase in the proportion employed (workers per assignee) and the 1.4 percent increase in weeks worked per worker. Overall, there was no change in earnings per hour worked and virtually no change in hours worked per week worked.

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[18] See Appendix D for an explanation of the procedures used to estimate impacts on these outcomes.

It is important to recognize that the finding of no effect on hourly earnings applies to the overall average for all workers in the treatment group. Program-induced changes in the composition of the group who worked may mask impacts on the average hourly earnings of subgroups of workers. For example, the program may have increased the hourly earnings of those who would have worked anyway but enabled more low-wage workers to become employed. The latter impact could have offset the former in its effect on the overall average hourly earnings of workers. Thus, the impact findings in Exhibit 5.6 do not necessarily reflect the impacts of JTPA Title II-A programs on the productivity of adult men.

Appendix G describes a separate, nonexperimental analysis of "latent wage rates" that we conducted to explore this issue further. Estimates of *latent wage rates* are designed to measure the productivity of individuals, including both those who were employed (whose wage rates can be observed) and those who were not employed (whose wage rates cannot be observed). Findings from this analysis suggest that access to JTPA had very little effect on the latent wage rates of adult male assignees in the sample. They therefore support the interpretation of a pure employment effect. For reasons discussed in the appendix, however, the results of the latent wage rate analysis may not be reliable.

Impacts on Earnings, Employment, and Earnings Components: Adult Men Recommended for Each Service Strategy

The preceding impact estimates, for all adult men in the 18-month study sample, were averages for many different types of men recommended by program staff for a variety of program services. This section focuses more narrowly on the subgroups of adult men recommended for each of the three clusters of services, or *service strategies*, defined in Chapter 2: the classroom training, OJT/JSA, and other services strategies.

As described in Chapter 2, the service strategies were defined in terms of the JTPA services recommended for sample members before they were randomly assigned to treatment or control status. Using recommended services, instead of services actually received, as the basis for this definition was necessary to allow us to identify the control group counterparts for the treatment group members in each service strategy subgroup.

Because recommended services were a good predictor of services received, this approach produced service strategy subgroups that did indeed receive distinctly different JTPA services. But recommended services were not a perfect predictor of services received, and more than one service was recommended for many sample members. Each service strategy subgroup in fact received a *mix of services*, not a single isolated service. Hence, the impact findings reported for each service strategy subgroup reflect the effects of the mix of services each subgroup received, not the impact of a specific program service. More specifically, as indicated earlier, our impact estimates reflect the incremental effect of the difference between the mix of services received by the treatment group and the mix received by the control group.

In addition, it is important to note that the kinds of men recommended for each service strategy differed. Thus, any differences in program impacts across service strategy subgroups cannot be attributed solely to differences in the kinds of services received. The impact findings in this section must therefore be viewed separately—as indicators of how well each strategy worked for the distinct group of people it actually served. Differences in impacts across service strategies do not indicate what would happen if a particular group were shifted from one service strategy to another.

We first describe how the employment and training services received by men varied across the three service strategy subgroups. We then examine the differences in services received between the treatment group and control group within each subgroup. The cross-strategy comparisons refer back to the comparisons in Chapter 3 showing how the subgroups differed on this dimension, while the treatment-control group comparisons reflect the increment in services attributable to JTPA Title II-A. It is this latter difference in service receipt between the treatment and control groups that is the *source* of program impacts for each service strategy subgroup. As part of this discussion, we also present estimates of program impacts on the attainment of a high school diploma or GED certificate associated with participation in school or training.

We then examine how the three service strategy subgroups differed from one another in terms of their baseline characteristics. The remainder of the section presents the estimated program impacts on each subgroup, based on the same measures of earnings, employment, and earnings components as those in the earlier sections on adult men overall.

DIFFERENCES IN EMPLOYMENT AND TRAINING SERVICES RECEIVED: TREATMENT AND CONTROL GROUPS WITHIN EACH SERVICE STRATEGY SUBGROUP

Recall from Exhibit 3.12 in Chapter 3 that 71.2 percent of the adult male treatment group members recommended for the classroom training service strategy became enrolled in JTPA Title II-A, whereas 56.6 percent of those recommended for the OJT/JSA strategy and 58.9 percent of those recommended for the other services strategy became enrolled in the program. Thus, the majority of adult male treatment group members enrolled in JTPA (60.8 percent overall), but the enrollment rates varied substantially between the classroom training subgroup and the other two service strategy subgroups.

Moreover, the three service strategy subgroups differed substantially in the specific JTPA services they ultimately received. Specifically, as Exhibit 3.19 indicated, 85.5 percent of the adult male *enrollees* recommended for the classroom training strategy received one or both of two JTPA services: classroom training in occupational skills and basic education. In the OJT/JSA subgroup, 86.5 percent of enrollees received on-the-job training, job search assistance, or both, and 88.7 percent of those enrollees recommended for the other services strategy received job search assistance, miscellaneous services, or both.

Thus, in terms of the services received by treatment group members, each service strategy rests mainly on two specific JTPA services, the defining service for that strategy and a secondary service. To the extent that treatment group members received the defining service for their strategy, the result reflects the primary service recommendation of SDA staff.

But the impact of JTPA depends on the *difference* between the services treatment group members received after being allowed to enroll in JTPA and the services they would have received if they had been excluded from the program. As discussed in Chapter 4, this difference is represented by the difference in services received, on average, between treatment group members and control group members.

Exhibit 5.7 displays estimates of this service difference for men who were recommended for each service strategy. The exhibit compares both the percentage of treatment group members and control group members who received each service (their *likelihood* of receiving the service) and the average number of hours of each service received (the *amount* of the service they received).

It is important to note that, for reasons discussed in Chapter 4, estimates for different services were obtained from different data sources and are subject to different limitations. Estimates of the receipt of *classroom training in occupational skills* and *basic education*, for example, include services from both JTPA and non-JTPA providers, measured from responses to the First Follow-up Survey. The estimates for these two services therefore offer a reasonably complete treatment-control group comparison.[19]

Estimates of the receipt of *on-the-job training* and *work experience*, on the other hand, apply to JTPA Title II-A services only and were obtained from SDA enrollment and tracking data. Because these services are typically provided only through JTPA, the estimates for each offer a fairly reliable treatment-control group comparison.

Job search assistance and *miscellaneous services*, however, are typically available from non-JTPA providers and thus are accessible to control group members. Because SDA

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[19] The survey-based estimates of receipt of classroom training in occupational skills and basic education are somewhat inconsistent with the SDA data on service receipt shown in Chapter 3, and they may in fact *understate* the incidence of these services. An analysis of these inconsistencies will be presented in our forthcoming final report.

Exhibit 5.7 Receipt of Employment and Training Services: Treatment Group and Control Group, by Service Strategy Subgroup

	Percentage receiving service			*Mean hours of service per sample member*		
Specific program service	*Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Treatment group (4)*	*Control group (5)*	*Difference, in hours (6)*
			Classroom training subgroup			
Classroom training in occupational skills[a] **	40.1%	24.2%	16.0%	235	140	95
Basic education[b] *	10.0	4.9	5.1	38	29	9
On-the-job training ** (JTPA only)	5.4	0.2	5.2	38[c]	1[c]	37[c]
Work experience ** (JTPA only)	1.7	0.0	1.7	8[c]	0[c]	8[c]
Job search assistance*** (JTPA only)	12.4	--	--	--	--	--
Miscellaneous[d] *** (JTPA only)	9.7	--	--	--	--	--
			OJT/JSA subgroup			
Classroom training in occupational skills[a] **	9.2%	9.2%	0.0%	58	55	3
Basic education[b] *	3.9	4.1	-0.3	11	13	-2
On-the-job training** (JTPA only)	26.6	0.5	26.1	119[c]	5[c]	114[c]
Work experience** (JTPA only)	2.4	0.0	2.4	10[c]	0[c]	10[c]
Job search assistance*** (JTPA only)	30.2	--	--	--	--	--
Miscellaneous[d] *** (JTPA only)	6.8	--	--	--	--	--

(Continued)

administrative records were our only reliable source of data on receipt of these services, we were unable to produce estimates for control group members. Exhibit 5.7 therefore does not allow for treatment-control group comparisons for these two categories of services.

The average number of hours of service receipt in columns 4 through 6 include the experience of both sample members who received the service and those who did not. To calculate an estimate of the average hours of receipt for only those sample members who actually received a service (service recipients), simply divide the estimate in column 4 *or* 5 (hours for all treatment group *or* control group members in the subgroup) by the estimate in column 1 *or* 2 (divided by 100).

Exhibit 5.7 Receipt of Employment and Training Services: Treatment Group and Control Group, by Service Strategy Subgroup (continued)

	Percentage receiving service			*Mean hours of service per sample member*		
Specific program service	*Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Treatment group (4)*	*Control group (5)*	*Difference, in hours (6)*
			Other services subgroup			
Classroom training in occupational skills[a] +	11.6%	9.9%	1.7%	73	77	-4
Basic education[b] +	5.7	4.5	1.2	18	10	8
On-the-job training ++ (JTPA only)	4.9	0.4	4.5	29[c]	2[c]	27[c]
Work experience ++ (JTPA only)	0.9	0.0	0.9	6[c]	0[c]	6[c]
Job search assistance +++ (JTPA only)	25.8	--	--	--	--	--
Miscellaneous[d] +++ (JTPA only)	29.0	--	--	--	--	--

Sources:

+ Unadjusted frequencies in this row are based on First Follow-up Survey data on receipt of the service from any provider.

+ + Unadjusted frequencies in this row are based on enrollment and tracking data from the 16 SDAs, the best available data on receipt of this service. Although the data are for JTPA Title II-A-funded services only, this service is typically not funded by non-JTPA providers.

+ + + Unadjusted frequencies in this row are also based on SDA enrollment and tracking data. Comparable data on receipt of this service from other providers were not available; nor were comparable data on receipt by control group members.

Notes: Sample size, classroom training subgroup: treatment group = 732, control group = 325; OJT/JSA subgroup: treatment group = 1,516, control group = 734; other services subgroup: treatment group = 732, control group = 380. Because of missing data, sample sizes for services calculated from different data sources may vary. Tests of statistical significance were not performed for this exhibit.

[a] Lasting longer than one week.

[b] Lasting longer than one week. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

[c] Hours, assuming a full-time job at 40 hours per week.

[d] "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

Before turning to the findings in Exhibit 5.7, note that one cannot sum the percentages receiving services in each column because individual sample members might have received more than one service.

The Classroom Training Subgroup. As shown in the first panel of the exhibit, although *all* men in this subgroup were recommended for classroom training in occupational skills, only 40.1 percent of the treatment group reported receiving this service. The principal reason why some treatment group members did not receive this service was that 28.8 percent of those in the classroom training subgroup never enrolled in JTPA (Exhibit 3.12 in Chapter 3). As expected, a number of control group members received instructional services, because non-JTPA funding was readily available for these services and because, as demonstrated by their application to JTPA, control group members were motivated to seek out these resources.

JTPA was therefore not the "only game in town" for instructional services. Nevertheless, the estimates in the first panel of the exhibit show that the program was an incremental source of those services. In the classroom training subgroup, treatment group members were more likely than control group members to receive classroom training in occupational skills (40.1 percent of the treatment group versus 24.2 percent) as well as more likely to receive basic education (10.0 percent versus 4.9 percent).[20] The treatment group was also more likely than the control group to receive on-the-job training and work experience.

The last three columns in the first panel indicate that the average differences in the amounts of services received between *all* members of the treatment group and the control group were 95 additional hours of classroom training in occupational skills and 9 additional hours of basic education—additional hours of service receipt attributable to the treatment group's having access to JTPA. These differences were mainly a consequence of the difference in the likelihood of receiving each service, and not primarily a consequence of a difference in the amount of the service received *per service recipient*.

To see this, note that among service recipients only, the average hours of receipt of classroom occupational skills training were very similar for the treatment group, at 586, and the control group, at 579 hours.[21] Moreover, although we have seen that the treatment group overall was more likely than the control group to receive basic education, control group *recipients* actually received a greater amount of basic education, at 592 hours, than treatment group recipients of this service, at 380 hours, on average. The content and quality of instruction may, of course, have differed between treatment and control groups.

Finally, 12.5 percent of the treatment group in the classroom training subgroup received job search assistance, and 9.7 percent received miscellaneous services, the last two categories in the panel.

The OJT/JSA Subgroup. In the OJT/JSA subgroup 27.1 percent of the treatment group received on-the-job training, and 30.7 percent received job search assistance, as shown in the second panel of Exhibit 5.7. Again, the main reason why some treatment group members did not receive the defining service for the strategy—OJT—was that 43.4 percent of the treatment group in this subgroup were never enrolled in JTPA during the follow-up period (Exhibit 3.12 in Chapter 3).

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[20] As previously noted, these survey-based estimates may understate the numbers of men who received classroom training and basic education.

[21] These numbers are derived by dividing the average number of hours of service receipt per treatment or control group member (column 4 or 5) by the corresponding proportion receiving the service (column 1 or 2, divided by 100).

As expected, however, the likelihood of receiving on-the-job training was much higher for men in the OJT/JSA subgroup than for those in the other two service strategy subgroups. The likelihood of receiving job search assistance was also much higher for this subgroup than for the classroom training subgroup, but it was similar to the rate of receipt for the other services subgroup.

Regarding treatment-control group differences, recall that we do not have data on receipt of on-the-job training from non-JTPA providers; but since few of those providers offer OJT, the estimates of treatment-control group differences in OJT receipt from JTPA providers are probably reasonably reliable. If that is the case, the estimates in the second panel of Exhibit 5.7 suggest that in the OJT/JSA subgroup, treatment group members were more likely than control group members to receive on-the-job training (27.1 percent versus 0.5 percent).

Column 6 also indicates that JTPA provided an additional 114 hours of OJT, averaged over *all* treatment group members. But among service recipients only, the average number of hours of OJT receipt was higher for the control group than for the treatment group.[22] Thus, similar to the case for adult men in the classroom training subgroup, the positive treatment-control group difference in hours of OJT receipt shown in column 6 was primarily the consequence of the difference in the *likelihood* of receiving OJT, not of the difference in the *amount* of the service received by those who received it.

In the OJT/JSA subgroup, treatment-control group differences in the receipt of classroom training in occupational skills and basic education were negligible, in terms of both the likelihood and the amount of the service received overall, and in terms of the average number of hours of the service received by service recipients only. As noted above, data limitations precluded our calculating estimates of differences in the receipt of the last two categories, job search assistance and miscellaneous services.

The Other Services Subgroup. Because the same data limitations apply to the other services subgroup, it was not possible to calculate treatment-control group comparisons for these last two categories of services. We do know from Chapter 3, however, that 58.9 percent of adult male treatment group members recommended for the other services subgroup subsequently enrolled in JTPA (Exhibit 3.12) and that 88.7 percent of those who enrolled received job search assistance, miscellaneous services, or both (Exhibit 3.19).

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22. Among service recipients only, the average number of hours of service receipt was 439 (119 divided by 27.1/100) for the treatment group. It was 875 for the control group, based on the more precise estimates (4.74 hours divided by 0.542/100) underlying the rounded estimates in the exhibit.

As shown in the last panel of Exhibit 5.7, 25.8 percent of adult male treatment group members received job search assistance, and 29.0 percent received miscellaneous services. These were also the two most common services received by adult female treatment group members in the other services subgroup.

The only information available to assess treatment-control group differences in service receipt for the other services subgroup is that on the less common services received by this subgroup. As shown in the first four rows of the last panel, treatment group members were slightly more likely than control group members to receive classroom training in occupational skills, basic education, and work experience—and somewhat more likely than control group members to receive on-the-job training.

Nevertheless, because we cannot measure control group members' receipt of the two most common services received by adults in this service strategy subgroup—job search assistance and miscellaneous services—we cannot reliably measure the *overall* treatment-control group difference in employment and training services for members of the other services subgroup.

Summary of Differences in Service Receipt. The preceding comparisons of service receipt across service strategy subgroups and between the treatment and control groups within each subgroup can be summarized as follows:

- Many men in the treatment group did not receive the service recommended for them, either because they were never enrolled in JTPA or because they received a service other than the primary one recommended for them.

- Nevertheless, the three service strategy subgroups, which were formed on the basis of service recommendations by JTPA intake staff, do appear to represent distinctly different clusters of services actually received and are therefore useful for analyzing the effects of different service strategies.

- Within each service strategy subgroup most men who enrolled in JTPA received one or both of two key services, which varied by service strategy: In the classroom training subgroup these were classroom training in occupational skills and basic education; in the OJT/JSA subgroup they were on-the-job training and job search assistance; and in the other services subgroup they were job search assistance and miscellaneous services. In all three subgroups the main reason why a portion of the treatment group did not receive one of the two key services in the strategy was failure to enroll in JTPA.

- The services actually received by each service strategy subgroup were consistent with the recommendations of intake staff to a considerable extent, but they differed in some important ways. Most important was the finding that only 27.1 percent of adult male treatment group members recommended for OJT (the OJT/JSA service strategy) actually received that service. Because 30.7 percent of adult male treatment group members recommended for OJT received job search assistance, it is most appropriate to characterize the service strategy as one based on placement in employment, with or without subsidized training.

- Treatment-control group differences in the average *amount* of service received were relatively modest, where those differentials could be measured. The average amount of additional occupational skills training received by treatment group members in the classroom training subgroup was only 95 hours. Similarly, the treatment-control group difference in receipt of OJT by men in the OJT/JSA subgroup was only 114 hours. These modest service differentials reflect the fact that not all treatment group members received these services and, in the case of occupational skills training, that some control group members received the service.

It is therefore important to note that the overall magnitude of the program impact does not reflect the effect of JTPA services versus no employment and training services. Rather it reflects the effect of the increment in services received by treatment group members beyond what they would have received without JTPA. Furthermore, when comparing the magnitudes of two service strategy subgroups estimated impacts, bear in mind that they do not represent impacts of two specific program services, but rather impacts of two different *service mixes*.

The costs of these different service mixes may vary considerably. And most importantly, the treatment-control group differential in service costs may vary substantially across the service strategy subgroups. Thus, the differences in impacts across service strategy subgroups that we report later in this section may not fully correspond to their differences in cost-effectiveness. As mentioned earlier, our forthcoming final report will present a benefit-cost analysis that takes these cost differences into account.

Impacts on High School Attainment. A number of men in the sample achieved a high school diploma or GED certificate as part of the school or training services described above. We refer to this result as the *attainment of a training-related high school credential*. Exhibit 5.8 shows the percentage of treatment group and control group members who reported both participating in school or training and attaining a high school diploma or GED certificate at some time during the 18-month follow-up period. The treatment-control group difference in this outcome represents the additional educational attainment attributable to having access to JTPA.

Estimates of this impact are presented for the target group of adult men as a whole and for high school dropouts only within each service strategy subgroup. Focusing only on high school dropouts isolates the impact of the program on those sample members for whom this was a potential impact. The estimated impact for the entire treatment group, on the other hand, indicates the relative importance of this outcome for the group as a whole.

As shown in the last panel in Exhibit 5.8, overall 3.8 percent of adult male assignees and 2.0 percent of adult male control group members attained a training-related high school credential. The statistically significant 1.9 percentage point difference represents the impact of JTPA on this outcome for all JTPA assignees. Among high school dropouts only, 12.7 percent of the assignees and 6.7 percent of the control group attained a training-related high school credential. Hence, JTPA had a statistically significant impact of 6.0 percentage points on adult male high school dropouts.

But these overall findings for men mask important variations among the three service strategy subgroups. The most striking findings are for classroom training, the strategy that involved the most basic education (see Exhibit 5.7) and that was therefore the most likely to lead to attainment of a training-related high school credential. Of the high school dropouts in the classroom training subgroup, 27.3 percent of the treatment group attained a training-related high school credential, whereas only 11.3 percent of the control group did. Hence, JTPA produced a statistically significant 16.0 percentage point increase in the likelihood that the high school dropouts would attain a training-related high school credential. But, because high school dropouts comprised less than a quarter of the men in the classroom training subgroup (see column 1), the impact of JTPA for assignees overall was only a 4.5 percentage point increase.

The OJT/JSA subgroup and the other services subgroup both received less basic education than the classroom training subgroup, and both had a very small treatment-control group difference in the receipt of basic education (Exhibit 5.7). As a result, one would not expect these service strategies to affect appreciably the attainment of a high school credential.

As expected, the impact on the attainment of a training-related high school credential was negligible and not statistically significant for the other services strategy. But JTPA produced a statistically significant 4.0 percentage point increase for high school dropouts in the OJT/JSA subgroup, which translates into a 1.2 percentage point increase for all assignees. Thus, even though the impact was statistically significant, it was small in magnitude and therefore not necessarily inconsistent with the fact that treatment and control group members in the OJT/JSA service strategy were about equally likely to receive basic education.

Exhibit 5.8 *Impacts on Attainment of a Training-Related High School Diploma or GED Certificate: JTPA Assignees Overall and High School Dropout Subgroup, by Service Strategy Subgroup*

		Percentage attaining a training-related high school credential [b]		
	Sample size [a] *(1)*	*Assignees (2)*	*Control group (3)*	*Difference, in % pts. (4)*
		Classroom training subgroup		
Full sample	1,018	6.7%	2.2%	4.5%***
High school dropouts	234	27.3	11.3	16.0***
		OJT/JSA subgroup		
Full sample	2,124	2.6	1.5	1.2**
High school dropouts	683	8.4	4.4	4.0**
		Other services subgroup		
Full sample	1,044	3.4	2.8	0.6
High school dropouts	341	10.2	8.7	1.5
		All subgroups		
Full sample	4,186	3.8	2.0	1.9***
High school dropouts	1,258	12.7	6.7	6.0***

Sources: Unadjusted frequencies based on Background Information Form responses and First Follow-up Survey responses.

a. Treatment and control groups combined

b. "Attainment of a training-related high school credential" is defined as the combination of having received some school or training service and having attained a high school diploma or General Educational Development certificate at some time during the 18-month follow-up period.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Differences in Baseline Characteristics across Service Strategy Subgroups: JTPA Assignees

As explained in Chapter 2, JTPA staff recommended sample members for the three service strategies on the basis of their individual work experience and educational needs, as well as their personal preferences. Variations in impacts among the three service strategy subgroups will therefore reflect not only differences in services received by these three groups, but also differences in their personal characteristics.

It is therefore important to recognize that the three service strategy subgroups differ. As shown in Exhibit 5.9:[23]

- The *classroom training subgroup* was the best educated group; only 25.2 percent did not have a high school diploma or GED certificate upon application to JTPA

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[23] For a more detailed description of the baseline characteristics of these subgroups, see Bloom (1991).

Exhibit 5.9 Selected Baseline Characteristics: JTPA Assignees, by Service Strategy Subgroup

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
Ethnicity				
White, non-Hispanic	57.0%	46.1%	68.9%	46.0%
Black, non-Hispanic	28.8	39.5	17.6	39.5
Hispanic	9.7	8.7	9.7	10.7
American Indian or Alaskan Native	1.6	1.3	1.6	1.4
Asian or Pacific Islander	2.9	5.1	1.5	3.4
Barriers to employment				
Receiving cash welfare[a]	14.6%	16.3%	16.6%	9.4%
No high school diploma or GED certificate	31.2	25.2	32.4	34.6
Worked less than 13 weeks in past 12 months	41.5	39.8	40.0	46.3
Number of barriers				
None of the above	37.7	41.2	38.0	33.4
One of the above	41.1	38.6	40.5	44.8
Two of the above	17.9	17.7	17.5	18.7
All three of the above	3.4	2.5	3.9	3.1
Work and training histories				
Ever employed	91.3%	90.4%	93.9%	87.4%
Mean individual earnings in past 12 months	$3,948	$3,980	$3,994	$3,827
Hourly earnings in most recent job				
Never employed	8.7%	9.6%	6.1%	12.6%
Less than $4	19.8	21.5	21.8	14.5
$4 or more	71.5	68.9	72.1	72.9
Employed upon application	13.1	14.8	13.0	11.8
Previously received occupational training	47.2	46.7	45.8	50.3
Public assistance status				
Receiving any public assistance[b]	37.0%	39.2%	38.8%	31.7%
Receiving AFDC	6.0	7.8	6.5	3.4
Receiving food stamps	28.7	29.3	30.9	24.0
Receiving other public assistance[c]	18.9	21.7	19.0	16.0

(Continued)

Exhibit 5.9 Selected Baseline Characteristics: JTPA Assignees, by Service Strategy Subgroup (continued)

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
AFDC history				
Never AFDC case head	91.5%	90.3%	91.1%	93.1%
AFDC case head less than 2 years	6.9	8.3	6.6	6.2
AFDC case head 2 years or more	1.7	1.4	2.3	0.7
JTPA required for welfare, food stamps, or WIN program [d]	9.6%	9.1%	11.3%	6.8%
Household composition				
No spouse or own child present	55.8%	53.5%	52.6%	59.2%
Own child under age 4, no spouse, present	3.8	5.2	2.6	2.8
Own child, none under 4, no spouse, present	4.6	3.6	3.8	4.6
Spouse present, with or without own child	35.8	37.8	41.0	33.4
Family income in past 12 months				
< $3,000	35.8%	33.4%	34.5%	40.4%
$3,000 - $6,000	25.1	26.0	25.9	22.5
$6,001 - $9,000	16.2	16.8	16.5	14.6
> $9,000	23.1	23.8	23.2	22.4
Living in public housing				
Yes	630.0%	6.5%	5.6%	7.4%
No	93.7	93.5	94.4	92.6
Age at random assignment				
22 - 29	44.6%	48.1%	44.0%	42.4%
30 - 44	43.3	41.5	44.1	43.6
45 - 54	7.8	7.4	8.0	7.9
> 54	4.3	3.0	3.9	6.1
Mean	*33.1*	*32.2*	*33.1*	*33.8*
Sample size	*3,759*	*925*	*1,832*	*1,002*

Source: Unadjusted frequencies based on Background Information Form responses.

[a] AFDC, General Assistance, or other welfare except food stamps.

[b] "Any public assistance" includes the following sources of assistance: AFDC, food stamps, unemployment insurance, housing assistance, and other cash assistance.

[c] "Other public assistance" includes unemployment insurance, housing assistance, and other (non-AFDC) cash assistance.

[d] WIN is the federal Work Incentive program.

(versus 32.4 percent for the OJT/JSA and 34.6 percent for the other services subgroup). This subgroup was in-between the other two in terms of the proportion never previously employed (9.6 percent versus 6.1 percent and 12.6 percent). Along with the OJT/JSA subgroup, members of the classroom training subgroup were more apt (at about 22 percent) to have earned a low wage (less than $4 hourly) in their most recent job than members of the other services subgroup, and more apt (at about 40 percent) to be receiving public assistance than the other services subgroup. And as indicated in Exhibit 3.17 in Chapter 3, the classroom training subgroup was also the least employable or job-ready of adult men, as measured by the estimated average earnings of the control group over the 18-month follow-up period. Those estimates were $11,780 for the classroom training subgroup versus $12,456 and $12,516 for the other two subgroups, respectively.

- The OJT/JSA service strategy was recommended for almost half of the adult male sample, and as a result, the *OJT/JSA subgroup* was by far the largest of the three (Exhibit 3.16 in Chapter 3). It also had the highest proportion of white men (68.9 percent) and the lowest proportion of black men (17.6 percent); the other two subgroups were about 40 percent black. And as mentioned above, the OJT/JSA subgroup also had the lowest proportion of members who had never been previously employed (6.1 percent).

- The *other services subgroup* had the lowest educational level, with 34.6 percent without a high school credential (versus 25.2 percent and 32.4 percent for the other two groups). At 87.4 percent this subgroup was also the least likely to have been employed in the past; and its members earned the least, on average, over the 12 months preceding their application ($3,883 versus $4,000 and $4,011). The other services subgroup was also the most apt to have had a family income below $3,000 in the previous 12 months (40.4 percent versus 33.3 percent and 34.4 percent). On the other hand, this subgroup was by far the least likely to be receiving public assistance upon application to JTPA (30.7 percent versus about 40 percent for the other two groups).

In addition to these observed differences it is possible, indeed likely, that the screening process used to recommend sample members for the different program services that defined each service strategy produced groups that differed in important ways that were not observed.

Note, however, that the data in Bloom (1991) cover all JTPA applicants randomly assigned to treatment or control status, whereas Exhibit 5.9 includes only the adult male treatment group in the smaller 18-month study sample. Appendix A in the present report compares the treatment and control groups in this sample.

Hence, one could cannot distinguish between differences in program impacts among the three service strategies that were due to differences in their service mixes versus those that were due to differences in the types of persons they served.

As a result, one cannot use the estimated impacts for each service strategy to determine what would happen if a group of men were switched from one service strategy to another. As discussed in Chapter 2, to provide this information would have required a different experimental design that could only have been implemented within a special demonstration program and thus would not have reflected the experience of ongoing JTPA programs. The impact findings for each service strategy therefore indicate only the effectiveness of each for the group it was serving at the time of the study.

MONTHLY EARNINGS TRENDS: TREATMENT AND CONTROL GROUPS WITHIN EACH SERVICE STRATEGY SUBGROUP

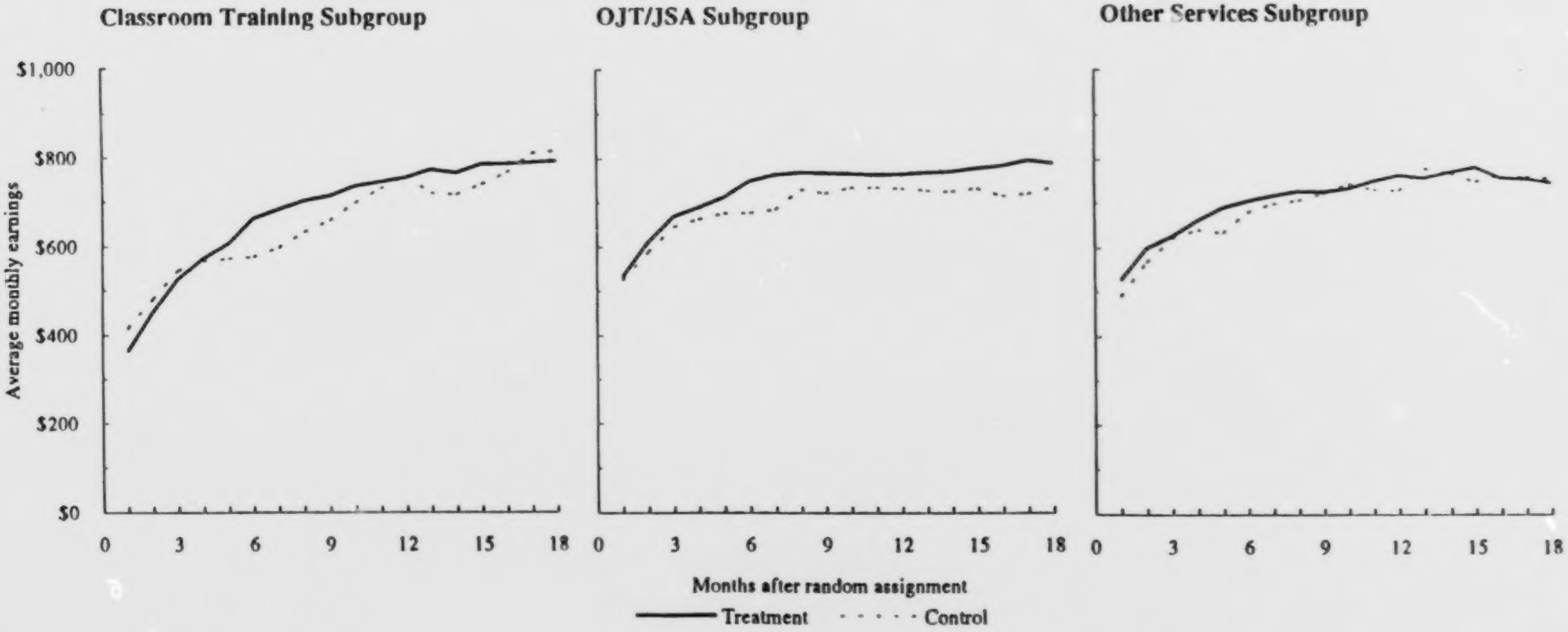
Exhibit 5.10 displays the monthly earnings trends of treatment and control groups in each service strategy subgroup over the course of the follow-up period. The earnings of all three control groups increased markedly over time; thus, we can conclude that the earnings of the treatment groups would have increased without access to JTPA. This outcome, once again, underscores the importance of comparing treatment group outcomes to a valid estimate of what they would have been without access to the program.

The largest and most consistent treatment-control group difference in earnings was in the OJT/JSA subgroup. This difference was negligible for the first several months, but thereafter the treatment group earned consistently and substantially more, on average, than the control group.

The pattern in the classroom training subgroup was quite different. There the treatment group earned less than the control group during the first several months, presumably while many treatment group members were in training and hence not available for full-time employment. For the next several months treatment group members earned more than control group members. But this advantage soon declined and fluctuated during the remainder of the follow-up period.

In the other service subgroup, there was very little difference between the monthly earnings of treatment group members and control group members throughout the follow-up period.

Exhibit 5.10 Average Monthly Earnings of Adult Men: Treatment Group and Control Group, by Service Strategy Subgroup



Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: treatment group = 733, control group = 325; OJT/JSA subgroup: treatment group = 1,516, control group = 734; other services subgroup: treatment group = 732, control group = 380. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

IMPACTS ON EARNINGS: JTPA ASSIGNEES AND ENROLLEES IN EACH SERVICE STRATEGY SUBGROUP

Exhibit 5.11 presents quarterly and 18-month impacts on the earnings of assignees and enrollees in each service strategy subgroup.[24] The OJT/JSA strategy appears to have been the most successful for the men it served.

There was a stable pattern of positive impacts on earnings for this subgroup, with no decline at the end of the period, as there was for the other two subgroups. The magnitude of the estimated impacts per JTPA assignee ranged from $54, or 3.1 percent, in the first quarter to $201, or 9.3 percent, in the sixth. The estimates were statistically significant at conventional levels in the third and sixth quarters.

The cumulative effect of these positive quarterly impacts for the OJT/JSA subgroup was an estimated impact on earnings of $781, or 6.3 percent, per JTPA assignee over all quarters. This result was statistically significant and implied a total impact per JTPA enrollee of $1,418.

This evidence of a positive impact on earnings is reinforced by the significant program impacts on employment in the OJT/JSA subgroup to be presented in the next subsection. It therefore appears that adult men recommended for this service strategy experienced a real program-induced increase in earnings that became salient during the second follow-up quarter, was sustained throughout the remainder of the follow-up period, and produced a moderate overall cumulative increase in earnings for the follow-up period.

In contrast, impacts on the earnings of men recommended for the other two service strategies were small, and inconsistent over time. The estimated total impact on earnings for all quarters was $418, or 3.5 percent, for men in the classroom training subgroup and $261, or 2.1 percent, for men in the other services subgroup. Neither estimate was statistically significant. Furthermore, no single estimate of a quarterly impact on earnings was statistically significant for either subgroup, and in both groups there was no clear pattern to the estimates.

IMPACTS ON EMPLOYMENT: JTPA ASSIGNEES IN EACH SERVICE STRATEGY SUBGROUP

Exhibit 5.12 presents estimates of JTPA impacts on employment, as measured by the *percentage employed*, the average number of *weeks worked*, and the average number of

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[24] As noted earlier, the earnings estimates include wages paid to JTPA participants in OJT positions. During the 18-month follow-up period the 16 SDAs reimbursed employers a total of about $700 per adult male *OJT participant*. Within the OJT/JSA subgroup the reimbursement was about $170 per adult male ***assignee*** over the follow-up period.

Exhibit 5.11 *Impacts on Earnings: JTPA Assignees and Enrollees, by Service Strategy Subgroup*

Period		Control mean	Impact per assignee		Inferred impact per enrollee, in $
			In $	*As % of (1)*	
		(1)	*(2)*	*(3)*	*(4)*
			Classroom training subgroup		
Quarter	1	$ 1,440	$ -101	-7.0%	$ -146
	2	1,714	126	7.3	183
	3	1,884	213	11.3	310
	4	2,184	50	2.3	73
	5	2,171	151	6.9	219
	6	2,387	-21	-0.9	-30
All quarters		11,780	418	3.5	608
			OJT/JSA subgroup		
Quarter	1	$ 1,757	$ 54	3.1%	$ 99
	2	2,014	135	6.7	244
	3	2,133	164*	7.7	297
	4	2,199	94	4.3	171
	5	2,183	133	6.1	241
	6	2,169	201**	9.3	366
All quarters		12,456	781*	6.3	1,418
			Other services subgroup		
Quarter	1	$ 1,677	$ 74	4.4%	$ 131
	2	1,951	104	5.3	185
	3	2,123	44	2.1	79
	4	2,199	44	2.0	78
	5	2,292	13	0.6	23
	6	2,274	-19	-0.8	-33
All quarters		12,516	261	2.1	463

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 732, control group = 325; OJT/JSA subgroup: assignees = 1,516, control group = 734; other services subgroup: assignees = 732, control group = 380. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

Exhibit 5.12 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: JTPA Assignees, by Service Strategy Subgroup*

	Percentage employed		*Weeks worked*		*Hours worked*	
Period	*Control mean* *(1)*	*Impact, in % pts.* *(2)*	*Control mean* *(3)*	*Impact, in weeks* *(4)*	*Control mean* *(5)*	*Impact, in hours* *(6)*
			Classroom training subgroup			
Quarter 1	50.5%	-1.3%	5.9	-0.8**	235	-25
2	55.3	3.8	6.5	0.2	270	16
3	60.7	4.6	7.1	0.5	293	29
4	68.4	1.2	7.8	0.3	326	12
5	72.0	0.8	7.8	0.6	315	33*
6	73.7	1.2	8.8	0.5	341	10
All quarters [a]	83.2	1.3	43.8	1.3	1,780	74
			OJT/JSA subgroup			
Quarter 1	56.9%	4.5%**	6.7	0.4	284	12
2	61.3	6.0***	7.6	0.6**	319	21*
3	63.1	6.5***	7.8	0.6**	329	26**
4	66.8	3.3*	8.0	0.2	338	10
5	69.3	1.4	8.0	0.4	334	20*
6	68.3	2.2	8.7	0.5	325	32***
All quarters [a]	83.8	3.9**	46.6	2.2**	1,929	121*
			Other services subgroup			
Quarter 1	65.9%	-1.8%	6.5	-0.2	264	-7
2	65.9	-0.2	7.2	0.1	301	-2
3	66.4	3.6	7.4	0.3	309	5
4	66.0	5.9**	7.3	0.6*	311	11
5	72.6	3.1	7.6	0.5	317	15
6	66.9	1.9	7.7	0.4	313	9
All quarters [a]	85.6	1.4	43.8	1.7	1,814	31

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 732, control group = 325; OJT/JSA subgroup: assignees = 1,516, control group = 734; other services subgroup: assignees = 732, control group = 380. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

[a] For columns 1 and 2 ("percentage employed") this row shows the percentage of control group members who reported being employed at any time during the follow-up period and the estimated impact on this percentage for assignees, respectively.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

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hours worked. All three sets of estimates indicate sizable and statistically significant impacts for the OJT/JSA service strategy subgroup and negligible impacts for the other two service strategies, mirroring the earnings results in the preceding subsection.

Specifically, estimates of program impacts on hours worked by men recommended for OJT/JSA (column 6) were statistically significant for all follow-up quarters and did not diminish over time. Indeed, as was the case for impacts on earnings, the largest impact on hours worked by this group was in the sixth follow-up quarter. The cumulative effect of these impacts was a statistically significant total increase of 121 hours per assignee. Estimates of impacts on weeks worked by this group (column 4) were statistically significant in two of the six quarters and also were consistent over time, with a statistically significant 2.2 week total increase per assignee for the full follow-up period. Finally, estimated impacts on the percentage employed were statistically significant in the first four quarters but were substantially smaller and not significant in the last two quarters. For the follow-up period as a whole the program increased the percentage of sample members employed by a statistically significant 3.9 percentage points.

In sharp contrast, the impact estimates for all three employment measures for the two other service strategy subgroups were small, and only 3 of the 42 estimates in columns 2, 4, and 6 were statistically significant.

IMPACTS ON THE COMPONENTS OF EARNINGS: JTPA ASSIGNEES IN EACH SERVICE STRATEGY SUBGROUP

Exhibit 5.13 presents, for each service strategy subgroup, estimates of JTPA impacts on the four components of earnings discussed earlier. Because impacts on earnings (column 1) were statistically significant only for OJT/JSA (see Exhibit 5.11), the following discussion considers only this service strategy subgroup.[25]

The 6.3 percent increase in total earnings during the follow-up period for the OJT/JSA subgroup was due mostly to the 4.7 percent increase in workers per assignee.[26] In other words, the earnings gain experienced by this treatment group reflected mainly an increase in the proportion who were able to find jobs. The other components of the group's earnings gain was a 1.5 percent increase in the average number of hours worked per week worked. In other

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[25] See Appendix D for a description of the method by which the estimates were calculated. Again, we did not calculate significance levels for the earnings components exhibits because the estimates in the last three columns were calculated indirectly.

[26] Note that the employment impact in Exhibit 5.13 is expressed as a percentage of the corresponding control group mean. In the preceding exhibit, 5.12 (column 2), the employment impact is expressed as a percentage point difference from the corresponding control group mean.

Exhibit 5.13 *Percentage Impacts on Earnings and Its Components: JTPA Assignees, by Service Strategy Subgroup*

Period	*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
			Classroom training subgroup		
Quarter 1	-7.0%	-2.6%	-10.5%	2.4%	4.3%
2	7.3	6.8	-3.2	2.3	1.4
3	11.3	7.6	0.0	2.1	1.2
4	2.3	1.7	1.7	0.1	-1.2
5	6.9	1.1	5.9	3.2	-3.2
6	-0.9	1.6	3.5	-2.1	-3.7
All quarters	3.5	1.6	1.3	1.2	-0.6
			OJT/JSA subgroup		
Quarter 1	3.1%	7.9%	-1.7%	-1.8%	-1.0%
2	6.7	9.8	-2.1	-0.8	0.0
3	7.7	10.3	-2.3	0.2	-0.3
4	4.3	5.0	-2.1	0.2	1.3
5	6.1	2.0	2.9	0.9	0.1
6	9.3	3.2	2.6	3.8	-0.6
All quarters	6.3	4.7	-0.1	1.5	0.0
			Other services subgroup		
Quarter 1	4.4%	-2.7%	-0.2%	0.3%	7.2%
2	5.3	-0.4	2.2	-2.4	6.0
3	2.1	5.4	-1.8	-1.8	0.5
4	2.0	9.0	-0.2	-4.8	-1.5
5	0.6	4.2	1.7	-1.2	-4.0
6	-0.8	2.8	2.8	-2.7	-3.5
All quarters	2.1	1.6	2.3	-2.2	0.4

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 732, control group = 325; OJT/JSA subgroup: assignees = 1,516, control group = 734; other services subgroup: assignees = 732, control group = 380. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is calculated as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

words, treatment group members who found employment tended to work slightly more hours per week. There was virtually no effect on the average earnings per hour worked.

Hence, the earnings gain experienced by the OJT/JSA subgroup was due entirely to an increase in the amount of time worked and not at all to an increase in the wages received for the time worked. As mentioned earlier, however, this type of finding for the subgroup overall could mask important compositional shifts within the group of men who worked, and it was not possible to control statistically for such potential compositional shifts (see Appendix G).

Impacts on Earnings: Adult Men in Selected Key Subgroups

This section presents estimates of JTPA impacts on the earnings of selected key subgroups of adult men defined to represent segments of the JTPA Title II-A population that have received attention in policy discussions about the program. Other key subgroups were defined to represent groups with different labor market experiences and those expected to have different needs for employment and training services.[27]

Specifically the analysis was designed to identify groups for which the JTPA Title II-A program was effective or ineffective at the 16 study sites. Distinguishing those groups for which the program was working can facilitate future research efforts to study the factors that lead to program success, while identifying groups for which the program was not working can help target efforts to improve it.

By itself, however, the analysis cannot determine why the program was effective for some groups and not for others. And it cannot yield simple prescriptions about how to improve the program. The analysis can only measure the effects of the program, the way it was actually operated, on the people it actually served.

Exhibit 5.14 presents the findings for key subgroups of adult men in the same way that Exhibit 4.15 in Chapter 4 did for subgroups of adult women. Each panel in the exhibit defines a set of subgroups in terms of a particular dimension, for example, ethnicity, selected barriers to employment, and work histories.

The first column in the exhibit presents the sample size for each subgroup, including both treatment group and control group members. The second column displays the mean earnings of control group members in each subgroup over the 18-month follow-up period. The third column presents the estimated impacts per JTPA assignee in each subgroup. Asterisks beside the impact estimates denote that the estimates were statistically significantly different from zero, whereas asterisks in the final row in each panel indicate that the subgroup impact estimates in the panel were statistically significantly different from one another.

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[27] We selected the subgroups examined in this section based on their relevance to policy discussions, before we calculated the estimates. In other words, we did not select them on the basis of the size or significance of the program effects presented below.

The fourth column displays the column 3 estimates adjusted for differences in the distribution of the subgroup across the 16 study sites. These adjusted estimates control statistically for the extent to which some subgroups were more heavily concentrated in sites with more or less positive impacts than others. The estimates in column 5 adjust the column 3 estimates for both differences in subgroup distributions across sites and differences in subgroup distributions across service strategies.[28]

To interpret the findings in the exhibit, one should proceed as follows. First, to assess the likely variation in impacts across subgroups, compare the impact estimates in column 3. Estimates that are statistically significant are those that are most likely to represent true impacts for a subgroup, as opposed to chance results due to random sampling error. In other words, one should place confidence in these estimates. Moreover, those panels that contain subgroup impact estimates that are statistically significantly different from one another provide the strongest evidence that the true impacts for the subgroups in the panel were actually different. If the subgroup impact estimates within a panel are not statistically significantly different, the fact that their point estimates vary does not offer sufficient evidence that the true impacts for the subgroups were actually different from one another.[29]

On the other hand, the fact that subgroup impact estimates are not statistically significant, or not statistically significantly different from one another, does not necessarily mean that the impacts or the impact differences do not reflect real impacts or real differences. Because of the small sample size of many subgroups, the possibility of random sampling error in the impact estimates for those subgroups is very high. In these cases the available data are not sufficient to determine with precision the magnitude of subgroup impacts or their differences.

Having examined the column 3 estimates for a set of subgroups in a panel, one should next read across the rows to columns 4 and 5. To the extent that the variation in impact estimates across subgroups changes as one moves from the column 3 estimates to the adjusted

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[28] The estimates in columns 4 and 5 adjust the distribution of each subgroup to equal the distribution of adult men overall across sites (column 4) or across sites and service strategies (column 5). For a full description of the methodology for deriving these estimates, see Appendix D.

[29] All the subgroup impact estimates in Exhibit 5.14 are based on ordinary least squares regressions on a pooled sample of all adult men, with the treatment indicator interacted with the defining characteristic of the subgroup and (as appropriate) site and site and service strategy. This approach allowed direct calculation of the F-test for differences in impacts among subgroups in each panel of the exhibit. Subgroup impacts were also estimated on samples containing only the subgroup of interest; in general, these estimates differed little from those based on the pooled regressions.

Exhibit 5.14 Impacts on Total 18-Month Earnings: JTPA Assignees, by Selected Key Subgroup

				Impact, in $, adjusted for sample distribution across	
Key subgroup, defined by:	*Sample size*[a] *(1)*	*Control mean (2)*	*Impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
Ethnicity					
White, non-Hispanic	2,668	$ 12,929	$ 625	$ 807*	$ 769
Black, non-Hispanic	1,155	10,931	957	73	183
Hispanic	400	13,555	-741	730	454
F-test, difference among subgroups			n.s.	n.s.	n.s.
Barriers to employment (in italic)					
Receiving cash welfare	611	9,541	-46	52	38
No cash welfare	3,788	13,032	624*	616	608
F-test, difference between subgroups			n.s.	n.s.	n.s.
No high school diploma or GED certificate	1,249	10,353	398	447	531
High school diploma or GED certificate	2,873	13,335	878**	923**	928**
F-test, difference between subgroups			n.s.	n.s.	n.s.
Worked less than 13 weeks in past 12 months	1,614	10,478	-210	-110	-91
Worked 13 weeks or more in past 12 months	2,392	14,320	787*	743	740
F-test, difference between subgroups			n.s.	n.s.	n.s.
Number of barriers					
None of the above	1,465	15,142	1,203**	1,199**	1,158*
One of the above	1,550	12,184	194	296	332
Two of the above	617	9,044	30	181	500
All three of the above	116	8,595	-146	-356	-1,248
F-test, difference among subgroups			n.s.	n.s.	n.s.
Work histories					
Never employed	365	8,813	142	217	482
Earned < $4 hourly in last job	728	8,692	1,372	1,406	1,475*
Earned $4 or > hourly in last job	3,326	13,640	470	482	454
F-test, difference among subgroups			n.s.	n.s.	n.s.
Employed upon application	619	13,618	2,093**	2,017**	1,761*
Not employed upon application	3,782	12,341	290	315	376
F-test, difference between subgroups			*	*	n.s.

(Continued)

Exhibit 5.14 *Impacts on Total 18-Month Earnings: JTPA Assignees, by Selected Key Subgroup (continued)*

				Impact, in $, adjusted for sample distribution across:	
Key subgroup, defined by:	*Sample size*[a] *(1)*	*Control mean (2)*	*Impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
Household composition					
No spouse or own child present	2,163	$ 11,022	$ 387	$ 403	$ 456
Own child under age 4, no spouse, present	133	13,709	-2,398	-2,380	-2,633
Own child, none under 4, no spouse, present	179	11,092	1,200	1,082	1,340
Spouse present, with or without own child	1,592	14,927	464	564	585
F-test, difference among subgroups	1,323	7,550	n.s.	n.s.	n.s.
Family income in past 12 months					
$6,000 or less	2,432	10,542	530	566	553
More than $6,000	1,736	15,750	30	-11	-76
F-test, difference between subgroups			n.s.	n.s.	n.s.
Living in public housing					
Yes	283	10,946	-495	-474	-319
No	4,021	12,595	695*	711**	685*
F-test, difference between subgroups			n.s.	n.s.	n.s.
Age at random assignment					
22 - 29	1,955	13,361	704	654	665
30 - 44	1,925	12,213	693	715	674
> 44	539	10,489	-537	-287	-297
F-test, difference among subgroups			n.s.	n.s.	n.s.
Recommended for JSA only					
Yes	335	13,727	-658	-286	n/a
No	4,084	12,425	640*	641*	n/a
F-test, difference between subgroups			n.s.	n.s.	n.s.
Full sample	4,419	12,306	550	550	550

Source: Estimates based on First Follow-up Survey responses.

Notes: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Control means were not regression-adjusted. Sample sizes for mutually exclusive subgroups within a panel do not necessarily sum to the sample size for the target group as a whole, because persons in omitted subgroups or with missing data on the variable used to define the subgroup are excluded.

a. Treatment and control groups combined.

b. AFDC, General Assistance, or other welfare except food stamps.

c. WIN is the federal Work Incentive program.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the F-test for the difference in impacts between or among the subgroups in the category is not statistically significant.

estimates in these last two columns, site-specific effects or site- and service strategy-specific effects explain part of the subgroup variation. In other words, the fact that one or more of the groups were concentrated in different sites or different service strategies is responsible for part of the variation in subgroup impacts. On the other hand, to the extent that the variation in impact estimates remains unchanged as one moves from the column 3 estimates to the adjusted estimates, differences in the distributions of the subgroups across sites or sites and service strategies do not explain the subgroup variations.

The following discussion of the findings in Exhibit 5.14 focuses primarily on those subgroup impact estimates that were statistically significantly different from zero and on those that were statistically significantly different from one another, because we can place the most confidence in these estimates. We begin, however, by discussing the first subgroup estimates in the exhibit, even though they were neither statistically significant nor significantly different from one another, because this lack of significance is an important finding to note. This set of estimates is for the major ethnic groups.

ETHNIC GROUPS

Perhaps the most widely discussed subgroups among the JTPA-eligible population are those defined in terms of their ethnic backgrounds. In particular, there has been considerable debate about the extent to which JTPA is serving white versus minority persons and about the extent to which the program is providing different types of services to each of these groups (see, for example, U. S. General Accounting Office, 1991).

As shown in the first four rows of Exhibit 5.14, the point estimates for ethnic subgroups differ, but the results do not indicate a consistent or statistically significant relationship between ethnic background and program impacts. None of the impact estimates in column 3 for whites, blacks, or Hispanics was statistically significant, nor were the three estimates statistically significantly different from one another. When the estimates for blacks and Hispanics are adjusted for their distribution across sites, the point estimates change, reflecting the relative concentration of the subgroups in some sites (especially Hispanics), but in neither column 4 nor column 5 were the three estimates statistically significantly different from one another,[30] even though the adjusted estimate in column 4 for white men was significant. All told, these findings do not provide strong evidence of major differences in the impacts for men in the three main ethnic groups.

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[30] Unlike the results for adult women in Chapter 4, the difference among the impact estimates for men in the three ethnic groups was not statistically significant at a near-conventional level (the .15 level, two-tailed test) either.

SUBGROUPS FACING SELECTED BARRIERS TO EMPLOYMENT

For this analysis we defined the selected barriers to employment facing sample members as welfare receipt (measured by whether they were receiving AFDC, General Assistance, or other welfare except food stamps when they applied to JTPA); limited education (measured by whether they lacked a high school diploma or GED certificate upon application), and limited recent work experience (measured by whether they had worked less than 13 weeks during the 12 months preceding their application).

The first finding to note here is the extent to which each employment barrier was reflected in the 18-month follow-up earnings of control group members and, hence, in what treatment group members would have earned without access to JTPA. As shown in column 2, control group members who faced an employment barrier, earned much less than their counterparts who did not face that barrier. Therefore, as was the case for adult women in Chapter 4, these barriers were indeed obstacles to employment.

Second, in each case those assignees who *did not* face the barrier in question experienced a statistically significant positive impact on earnings. These estimated impacts ranged from $624 to $878. In contrast, assignees who were receiving welfare upon application, assignees with limited education, and assignees with limited recent work experience did not, on average, experience a significant positive impact. This pattern did not change materially when the estimates were adjusted for the distribution of sample members across sites or across sites and service strategies.

In no case, however, was the *difference* between the estimated impacts for these pairs of subgroups statistically significant. Thus, the evidence suggests that men who were not facing a major employment barrier, and hence were presumably more job-ready than those who were facing such a barrier, experienced larger program-induced earnings gains, but we cannot be sure of these differences.

Because some men facing one of these barriers to employment were also facing one or both of the others, the three subgroups overlap. To distinguish among subgroups by the overall difficulty they faced in becoming employed, we also estimated impacts on subgroups facing none, one, two, or all three of the barriers, as displayed in the next four rows of the panel.

Once again, two findings stand out. First is the extent to which the barriers, as defined, predict the difficulty of becoming employed. Specifically, as the number of barriers increases, average control group earnings decrease, from $15,142 to $8,595. This consistent and striking pattern clearly illustrates the importance of these barriers in determining labor market success.

Second is the fact that the only one of these subgroups to experience a statistically significant positive impact on earnings was that which was facing none of the three employment barriers. The impact estimates for the other subgroups were small and in one case negative; and none was statistically significant. This pattern was similar when the findings were adjusted for the distribution of subgroups across sites or sites and service strategies.[31] Nevertheless, the difference in impact estimates across subgroups was not statistically significant. Thus, although we are confident there was a positive program impact on those men facing none of the three employment barriers, we cannot say this effect was greater than that on men who were facing one or more of the barriers.

SUBGROUPS EMPLOYED OR NOT EMPLOYED UPON APPLICATION TO JTPA

Other subgroup findings that showed some statistical significance were those for subgroups of men defined by their employment status upon application to JTPA. As shown in the last two rows of the next panel, the estimated impact for men who were employed when they applied was $2,093, whereas that for men who were not employed was $290. The first estimate was statistically significant, and the second was not; and the two estimates *were* statistically significantly different from each other. Moreover, the pattern is similar when the findings are adjusted for the distribution of sample members across sites or sites and service strategies (although the two estimates were not significantly different from each other in column 5). Thus, here we are fully confident that the program was working well for those men who were employed upon application and less well for those who were not.

SUBGROUPS LIVING IN PUBLIC HOUSING OR NOT

The large positive impact estimates for men who were not living in public housing when they applied to JTPA (4,021 persons) was statistically significant and varies little when it is adjusted for the subgroup's distribution across sites or sites and service strategies. There is thus strong evidence that JTPA produced a real, positive impact on earnings for this group and that the impact estimate does not reflect a differential distribution of the group across sites or service strategies.

In contrast, the impact estimate for the much smaller sample of men who were living in public housing when they applied to the program (283 persons) was negative and not statistically significant. This estimate and the adjusted estimates in columns 4 and 5 were not statistically significantly different from those for the other subgroup, however.

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[31] The shift in column 5 to a large negative impact for men with all three barriers to employment reflects a large amount of sampling error due to the very small sample upon which the estimate is based.

MEN RECOMMENDED FOR JOB SEARCH ASSISTANCE ONLY

In previous sections of this chapter we presented estimates of program impacts on the earnings of men recommended for each of the major JTPA service strategies. Here we present the estimated impact on the earnings of men recommended for job search assistance only. This low-intensity approach was recommended for about a third of the men in the other services subgroup.[32] As shown in the last panel of the exhibit, the estimated impact on the earnings of these men was statistically insignificant but negative, in contrast with the estimate for all other men. The difference in estimated impacts between the two groups was statistically insignificant, however, and the subgroup recommended for job search assistance only was too small to estimate the impact precisely.

A Summary of Impacts, in the Context of Previous Research

This final section summarizes the preceding findings on JTPA Title II-A impacts on the earnings, employment, and high school attainment of adult men. We first review the findings for adult men overall and compare them with results from previous experimental studies of employment and training programs for men. These findings serve as a benchmark for then considering our more detailed findings for men in the three service strategy subgroups and other selected key subgroups. Exhibit 5.15 displays our principal findings.

ADULT MEN OVERALL

Estimated impacts on the earnings of those adult men offered access to JTPA Title II-A programs averaged $550, or 4.5 percent of control group earnings, over the 18-month follow-up period. The program increased the percentage of these JTPA assignees who were employed at some time during the follow-up period by 2.8 percentage points; it increased the average number of weeks worked during the period by 4.8 percent; and it increased the average number of hours worked during the period by 4.5 percent.

The estimated impact on earnings was not statistically significant at conventional levels. But since the estimated impacts on all three employment outcomes were significant (.05 level), the estimated impact on earnings probably reflects a true impact, rather than a chance result due to random sampling error.

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[32] See column 1. As explained in Chapter 2, it was only in the other services subgroup that sample members could be recommended for job search assistance alone.

Exhibit 5.15 *Summary of Estimated JTPA Impacts on Earnings and Employment over the Full Follow-up Period: Adult Male JTPA Assignees in the 18-Month Study Sample, by Service Strategy Subgroup*

Impact per assignee on:	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
Earnings				
In $	$ 550	$ 418	$ 781*	$ 261
As % of control mean	4.5%	3.5%	6.3%	2.1%
Percentage employed [a]	2.8%**	1.3%	3.9%**	1.4%
Weeks worked				
In weeks	2.2**	1.3	2.2**	1.7
As % of control mean	4.8%	3.0%	4.7%	3.9%
Hours worked				
In hours	84**	74	121*	31
As % of control mean	4.5%	4.2%	6.3%	1.7%

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 732; OJT/JSA subgroup; assignees = 1,516, control group = 734; other services subgroup: assignees = 732, control group = 380. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. At any time during the 18-month follow-up period. The impact is measured in percentage points.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). For the impacts on earnings, weeks worked, and hours worked, the significance level of each estimate expressed "as % of control mean" is the same as that of the corresponding estimate expressed in dollars, in weeks, or in hours.

The impact on earnings appears to be attributable to an increase in employment among adult male assignees overall, and not to an increase in their average wage rates. Perhaps the simplest way to see this is to note that the impact on both average earnings and the average number of hours worked was 4.5 percent. Nevertheless, these findings for the target group overall could be masking important shifts within the group in terms of who became employed and how their wage rates did or did not change.

The impact findings are expressed as average treatment-control group differences, or impacts per JTPA assignee. They directly reflect the impact of randomly assigning sample members to the treatment group instead of the control group. But only 60.8 percent of adult male treatment group members actually became enrolled in JTPA, and 2.2 percent of the control group members also became enrolled. Furthermore, many control group members received services from other existing programs.

As a result, the impacts per JTPA assignee presented in this chapter do not reflect the impacts of receiving one or more employment and training services versus having received no such services. Instead, they reflect the impact of receiving some employment and training service versus receiving less service. Thus, the impact of JTPA depends not only on the JTPA services provided to men in the treatment group. It also depends on the services they would have received from other providers had they not had access to JTPA, which we measure by service receipt in the control group.

Direct evidence of the control group's receipt of non-JTPA services was available only for classroom training in occupational skills and basic education. In the classroom training subgroup, treatment group members were more likely to receive classroom training in occupational skills than control group members (40.1 percent versus 24.2 percent). They were also more likely to receive basic education (10.0 percent versus 4.9 percent).

We are also fairly confident that JTPA increased the receipt of on-the-job training in all three subgroups, since this service is seldom provided by non-JTPA programs. We were unable to measure the extent to which JTPA increased the receipt of job search assistance or miscellaneous services.

While access to JTPA substantially increased the *incidence* of receipt of occupational skills training, basic education, and OJT, the treatment-control differential in the average number of hours of occupational skills training *per assignee* for men in the classroom training subgroup was only 95 hours. Similarly, men in the OJT/JSA treatment group received only 114 more hours of OJT *per assignee* than their control group counterparts. These relatively small differentials reflect the fact that not all treatment group members received these services and, in the case of occupational skills training, that a nonnegligible proportion of control group members received services.

These service differentials led to a substantial increase in educational attainment for men in the classroom training subgroup, who were those most likely to receive occupational skills training or basic education. Twenty-seven percent of the high school dropouts in the classroom training treatment group attained a training-related high school credential, whereas only 11 percent of the control group members did so. Corresponding effects were much smaller for the other two service strategies, as would be expected from the nature of the services recommended for and received by members of these subgroups.

Exhibit 5.16 compares the estimates of impacts on earnings for adult men in the National JTPA Study with corresponding impact estimates for adult men from previous randomized experiments. The exhibit identifies each study, describes the adult men in its sample, indicates the types of program services they received, and presents estimates of program impacts. The impacts are stated as *quarterly* averages of the program impacts on the earnings of the treatment group, expressed in dollars and as a percentage of the control group mean. Separate impact estimates are presented for each of three years after random assignment, wherever available. To facilitate comparisons across studies, we converted all the estimates to constant July 1989 dollars.

Exhibit 5.16 Estimated Impacts on the Average Quarterly Earnings of Adult Men: The JTPA 18-Month Impact Analysis and Previous Experimental Studies

Program (Year evaluation began)	*Treatment group (1)*	*Program services (2)*	*Quarterly $ impact per treatment group member (% impact in parentheses)*		
			Year 1 (3)	*Year 2[a] (4)*	*Year 3 (5)*
National JTPA Study (1987)	Low-income men, 6% receiving AFDC; participation voluntary	Classroom training strategy: occupational skills training, basic education[b]	$72 (4%)	$65 (3%)	--
		OJT/JSA strategy: on-the-job training, job search assistance	$112 (6%)	$167* (8%)	--
		Other services strategy: job search assistance, miscellaneous[c]	$66 (3%)	$-3 (0%)	--
		All three strategies	$86 (4%)	$103 (5%)	--
San Diego I[d+] (1982)	AFDC applicants; participation mandatory	Job search assistance, unpaid work experience	$52 (4%)	--	--
San Diego Saturation Work Initiative Model[e+] (1985)	AFDC applicants and recipients; participation mandatory	Job search assistance, unpaid work experience, basic education,[b] occupational skills training	$142* (18%)	$125 (12%)	--
Texas Worker Adjustment Demonstration[f++] (1984)	Displaced workers; participation voluntary	Job search assistance, occupational skills training, on-the-job training	$197 (8%)	--	--
N.J. UI Reemployment Demonstration[g+++] (1986)	Displaced workers; Participation mandatory	Job search assistance	$239 (--)	--	--
		Job search assistance, occupational skills training, relocation assistance	$48 (--)	--	--
		Job search assistance early reemployment bonus	$150 (--)	--	--
National Supported Work Demonstration[h++++] (1976)	Low-income ex-offenders, 95% of whom were men, participation voluntary	Structured, paid work experience for up to 12 months	--[i]	$105 (7%)	$268 (17%)
	Low-income ex-addicts, 80% of whom were men; participation voluntary	Structured, paid work experience for up to 12 months	--[i]	$56 (4%)	$421* (32%)

Sources:

+Gueron and Pauly (1991), table 3.1, pp 86-87, and table 4.5, p. 161.

++Bloom (1990), table 8.1, p. 163.

+++Corson et al. (1989), table VI.3, pp. 285, 292, 297.

++++Manpower Demonstration Research Corporation (1980), tables 5-3 and 7-3, pp. 84 and 124; Kemper and Long (1981, table VIII. 3, p. 219); and Kemper, Long, and Thorton (1981, table V. 1 and VII. 1, pp. 131 and 157).

Notes:

The reported estimates from previous studies have been converted to July 1989 dollars. The " % impact" is the "$ impact" expressed as a percentage of the control group mean. Where dollar estimates appear without the corresponding percentage impacts, information on the control group was not available from the source cited.

(Continued)

The existing previous experimental studies of employment and training programs serving men are limited to three subpopulations: low-income men who were receiving or had applied for AFDC; displaced workers, who had lost formerly well-paid, stable jobs permanently because of changing technology or increased foreign competition; and low-income men who were ex-offenders or ex-addicts.

As shown in the second panel of Exhibit 5.16, two experimental studies focused on the effect of employment and training programs for men who had applied for AFDC-UP, the component of AFDC for two-adult households with unemployed parents. The Employment Preparation Program/Experimental Work Experience Program (commonly known as San Diego I) and the Saturation Work Initiative Model (SWIM) were both conducted in San Diego. Both programs provided a sequence of job search assistance followed by unpaid work experience for participants who did not find jobs. SWIM also provided basic education and occupational skills training for participants who did not become employed. The more intensive program, SWIM, produced quarterly impacts on treatment group earnings that were noticeable in the two years after random assignment and were statistically significant in the first year (at $142). The impact for San Diego I, the less intensive program, was small ($52) and not statistically significant in the first year—the only year for which impact estimates are available.

The next panel presents information on the two experimental studies of male displaced workers. The Texas Worker Adjustment Demonstration, conducted in Houston and El Paso, provided a sequence of job search assistance followed by classroom training in occupational skills or on-the-job training for participants who did not find employment. The New Jersey

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a. The estimates from the National JTPA Study (first panel) in this column are for the first two quarters only.

b. Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

d. "San Diego I" is the common abbreviation of the San Diego Employment Preparation Program/Experimental Work Experience Program. Conducted at all seven welfare offices in San Diego County, the program had a net cost (in nominal dollars) per treatment group member of $636.

e. The San Diego SWIM demonstration was conducted at two of the welfare offices in the county, at a net cost (in nominal dollars) per treatment group member of $919.

f. The Texas demonstration was conducted at one site in Houston and two sites in El Paso. Comparable information on the net cost of the program was not available from the source cited.

g. The New Jersey demonstration was conducted at 10 unemployment insurance offices in the state. Comparable information on the net cost of the program was not available from the source cited.

h. Included demonstrations in seven sites for ex-offenders and four sites for ex-addicts, with a net cost per treatment group member (in 1976 dollars) of $7,437 for ex-offenders and $6,185 for ex-addicts, or $8,843 and $7,776 including revenues generated by the demonstration.

i. Impact estimates are not reported for year 1 after random assignment because most treatment group members were in the program and thereby receiving heavily subsidized earnings in that year.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

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Unemployment Insurance Reemployment Demonstration Project, conducted at 10 UI offices in the state, offered three different types of programs: job search assistance only; job search assistance combined with occupational skills training or relocation assistance; and job search assistance combined with a cash bonus for early reemployment. Estimated program impacts in the first year were modest but not statistically significant for the Texas program ($197) and for all three components of the New Jersey program (ranging from $48 to $239).

The last study shown in the exhibit, the National Supported Work Demonstration, examined the impacts of a lengthy and intensive program of subsidized work experience for adult male ex-offenders and ex-addicts. Because the program lasted more than a year for many sample members, only the findings for the second and third years after random assignment represent post-program impacts. These impacts were small and not statistically significant in the second year after random assignment ($105 and $56) and larger for both groups, although still not statistically significant for ex-offenders, in the third year ($268 to $421).

The impacts on treatment group earnings for adult men in the National JTPA Study are reported on a quarterly basis in the exhibit to facilitate comparisons with the findings from the other studies. The year 1 estimates are based on findings for the first four follow-up quarters, but the year 2 estimates are based on findings for the fifth and sixth follow-up quarters only. The estimated impacts for men in the full sample (all three service strategy subgroups) were $86 per quarter during the first follow-up year and $103 per quarter during the first half of the second year. These impacts are toward the low end of the findings from the previous studies.

One must exercise caution in making these comparisons, however, because adult men in the JTPA sample differed substantially from those in the previous studies. For example, only 6 percent of the men in our 18-month study sample were receiving AFDC upon application to JTPA, and the AFDC recipients in the San Diego programs were probably less job ready than the typical adult male in JTPA Title II-A. At the other extreme, the displaced workers in the Texas and New Jersey demonstrations had, by definition, more extensive employment experience than the typical Title II-A participant. (This difference was acknowledged by the establishment of a separate JTPA title, Title III, to provide services for displaced workers.) Finally, although we do not know the proportions of ex-offenders and ex-addicts among the men in our sample, we must assume they were small by comparison to those in Supported Work.[33] Moreover, the men in Supported Work received much more intensive services than those generally provided by JTPA. In the next subsection we will extend this comparison with previous studies in examining the findings for the three service strategy subgroups in our sample.

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[33] As shown in Exhibit 3.5 in Chapter 3, for example, only 8 percent of all terminees in JTPA Title II-A nationally during program years 1987-1989 were ever arrested.

MEN RECOMMENDED FOR EACH SERVICE STRATEGY

As summarized in the earlier Exhibit 5.15, among the three service strategy subgroups the estimated impacts for the OJT/JSA subgroup were by far the largest and the only ones that were statistically significant.

At $781, the estimated impact on total 18-month earnings for the OJT/JSA subgroup was much larger than the estimates for the other two subgroups and the only one that was statistically significant. The same is true of the estimated impacts on all three employment outcomes. And as shown in Exhibit 5.16, the average *quarterly* impacts on earnings for the OJT/JSA subgroup ranged from $112 in year 1 to (a statistically significant) $167 in the first half of year 2—toward the high end of the results from the previous experimental studies.

The earnings impact for the OJT/JSA subgroup as a whole was produced almost entirely by an increase in the amount of time worked, with virtually no impact on the average amount paid per hour worked. The simplest way to see this is to note that the impact on both average earnings and the average number of hours worked was 6.3 percent (Exhibit 5.15). Once again, however, it is important to note that this finding for the OJT/JSA subgroup as a whole may reflect program-induced shifts in the composition of the group that was employed. Hence, one cannot interpret the finding to mean that the subgroup experienced no effects on wage rates.

The OJT/JSA impacts reflect a modest difference in the services received by the treatment group and the control group: Although 27.1 percent of the treatment group received on-the-job training from JTPA and only 0.5 percent of the control group did so, this produced a difference of only 114 hours of OJT receipt per sample member between the two groups. Because JTPA is typically the only provider of on-the-job training, this finding is probably a fair estimate of the actual difference in receipt of OJT.

In addition, 30.7 percent of the OJT/JSA treatment group received job search assistance from JTPA. Because JTPA is typically not the only source of this service, many control group members probably received this service from non-JTPA sources. There was, however, no adequate way to measure receipt of this service by control group members, and so we were unable to determine the corresponding treatment-control group difference.

MEN IN SELECTED KEY SUBGROUPS

Few of the impact estimates for key subgroups of adult men were statistically significant and even fewer were statistically significantly different from each other, including those for different ethnic groups. Hence, the degree of uncertainty about these estimates and the relatively small magnitudes of most of them provided few indications that JTPA impacts varied systematically across subgroups. Nevertheless, one important pattern did emerge from the analysis: The largest JTPA impact on earnings was among those adult men who were the most job-ready.

This relationship between job-readiness and program impacts emerged from two of the subgroup analyses reported earlier in Exhibit 5.14. The first compared the estimated impacts on men who, upon application, were facing none or one or more of the three selected barriers to employment: welfare receipt, limited education, and limited recent work experience. Those men facing *none* of those barriers experienced a large, statistically significant impact on earnings of $1,203 over the 18-month follow-up period, whereas the estimates for those men facing one or more barriers were small and statistically insignificant.[34]

Second, the estimated JTPA impact was extremely large and statistically significant ($2,093 per assignee) for men who were employed when they applied to JTPA, whereas the impact was small and not statistically significant for men who were not employed at application. Moreover, the difference between these two estimates was statistically significant in the F-test.

The program also appeared to have served men who were not living in public housing better than it served men who were. But here the estimated impact on the earnings of the former ($695) was much smaller than in the preceding two examples, and the difference between the estimates for the two subgroups was not significant, in part because the sample of men who were living in public housing was small.

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[34] Because the difference in impacts among these groups was not statistically significant in the F-test, we cannot be sure the subgroups facing one or more barriers did not experience a positive impact. But because the estimate for the subgroup facing no such barrier was highly significant, we can be confident the estimate accurately reflects a positive impact on this subgroup.

6

Out-of-School Youths: JTPA Impacts at 18 Months

THIS chapter presents estimates of JTPA Title II-A impacts on out-of-school youths. These estimates provide the first reliable evidence of the effects of a wide range of Title II-A youth programs. They also represent an important contribution to the small number of experimental studies of employment and training programs serving young, economically disadvantaged Americans—who, because of their limited work experience, are a very different population from adults and therefore should be studied separately.

The 4,793 out-of-school youths in the 18-month study sample were 16 to 21 years old at their random assignment and formed 28 percent of the full sample.[1] With an average age of 19, this youth sample was over 50 percent white, 32 percent black, and 15 percent Hispanic.

These proportions were similar for both female and male youths, but on a variety of other characteristics the sample differed along gender lines. Starting with previous work experience, 23 percent of the females had never held a job before, whereas only 16 percent of the males had never worked before; and 48 percent of the females versus 35 percent of the males either had never worked or had earned less than $4 hourly on their most recent job. The females were somewhat better educated than the males, with 51 percent of the former holding a high school diploma or GED certificate versus only 41 percent of the latter. Female youths, however, were much more likely to be living in a household receiving public assistance than male youths, at 47 percent versus 30 percent.[2]

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[1] Among the 2,649 female youths in the 18-month study sample, 2,323, or 87.7 percent, responded to the First Follow-up Survey; among the 2,144 male youths in the sample, 1,773, or 82.7 percent, responded to the survey. The estimated impacts presented in this chapter are based on these respondent subsamples. The information on baseline characteristics in this paragraph is based on Background Information Form responses for all out-of-school youths in the 18-month study sample.

The largest differences between the two genders, however, were in the proportion who were single parents and in the proportion who had been arrested. On the one hand, female youths were almost nine times more likely to be a single parent then male youths (at 36 percent versus 4 percent); on the other hand, male youths were four times more likely than female youths to have been arrested before their application to JTPA (at 23 percent versus 6 percent).

As explained in Chapter 2, we had originally planned to analyze findings separately for white and minority youths. However, the impact findings presented in this chapter vary more by gender than by ethnicity.[3] We therefore structure this chapter to contrast the estimates for female youths and male youths. There is some precedent for this distinction; previous research on employment and training programs for disadvantaged out-of-school youths has found different program impacts for female and male youths.[4]

Moreover, our findings for youths served by JTPA Title II-A stand in sharp contrast to the generally positive impacts for adults presented in chapters 4 and 5. In short, the estimated impacts for out-of-school youths overall are negative, reflecting a combination of small, statistically insignificant negative impacts for female youths and large, statistically significant negative impacts for male youths, on average.[5]

The remainder of this chapter investigates these differential impacts in more detail. Because our splitting of the youth sample by gender resulted in twice as many findings to present as those in each of the two preceding chapters, we moved to Appendix H exhibits corresponding to those in chapters 4 and 5 that were not essential to the discussion here.[6] Otherwise the discussion roughly parallels the sequence of topics presented in those earlier chapters.

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[2] See Exhibit 3.14 in Chapter 3 for more detail.

[3] As shown in Exhibit H.21 in Appendix H, although the difference in impacts by gender was not statistically significant, the large, negative estimated impact for male youths was significant, whereas the small, negative estimated impact for female youths was not. These findings mean we have confidence that the estimate for male youths reflects the true impact, but we cannot be sure whether the estimate for female youths was significantly different from that for male youths. The corresponding estimated impacts by ethnicity were also not statistically significantly different from each other, but they displayed smaller differences in magnitudes and statistical significance levels than did the estimated impacts by gender.

[4] See, for example, Maynard (1980) and Cave and Doolittle (1991).

[5] The female and male youth samples in this chapter are about half the size of the samples of adult women and adult men. The two youth samples therefore have less statistical power, that is, they are less likely to yield impact estimates that are statistically significant. In our discussions of the findings here, we are therefore careful to say whether they were statistically significant or not.

More specifically, the first main section examines impacts on earnings for out-of-school youths overall and by gender and compares those findings with estimates for young adults, ages 22 to 29. The second section presents more detailed estimates of impacts on the earnings of female youths recommended for each of the three service strategies, while the third section does the same for male youths. The fourth section examines impacts on female and male youths in selected key subgroups of interest to policymakers and program planners. In the final section we summarize the chapter's findings and place them in the context of results from previous experimental research.

Impacts on Earnings: Out-of-School Youths Overall and by Gender

To clarify the differences between the findings for female and male youths, this section presents results for youths both overall and by gender.

The first subsection uses monthly data to explore earnings trends over time, while the second examines JTPA enrollment patterns over time to distinguish between the in-program and post-program periods for the treatment group. Trends in earnings and enrollment are of interest for three reasons. First, as we saw in the two preceding chapters, people tend to seek employment and training services at unusually low points in their earnings profiles, and so we can expect to see increases in control group earnings over time. Second, we also saw that some types of services (such as classroom training) involve an initial investment of time during which employment opportunities may be foregone, followed by a post-program period during which a return on that investment may emerge. Finally, among youths in particular, employment may increase over time as they mature.

The third subsection shifts to an examination of quarterly estimates, presenting the estimated impacts on earnings per *JTPA assignee* (treatment group member) and per *JTPA enrollee* (treatment group member who became enrolled in the program at some time during the 18-month follow-up period). The last subsection compares the findings for the two youth target groups with those for their counterparts among the young adults in the sample to help determine whether the differences between the findings for youth and adults reflects a continuous trend between age and program impacts or a sharp change between impacts for the adult and youth programs.

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[6] Supplementary exhibits in Appendix H for female youths are those numbered H.1 to H.6; for male youths, H.7 to H.12; and for all youths, H.13 to H.21.

MONTHLY EARNINGS TRENDS: TREATMENT GROUP AND CONTROL GROUP

The first graph in Exhibit 6.1 shows that for all out-of-school youths the earnings trends of the treatment and control groups were very different from those for adults shown in the two previous chapters. Here the curve for treatment group earnings was *below* the curve for control group earnings in most months; for adults the treatment group curve was above the control group curve in most months.[7]

The other two graphs in Exhibit 6.1 indicate that for female youths control group earnings exceeded treatment group earnings to a small degree in most months, but for male youths the difference was much more pronounced and sustained. Among the males, earnings in the treatment group were persistently below earnings in the control group, and the gap grew during months 6 to 9, declined somewhat through month 14, then grew again.

Indeed, as will be seen later, the treatment-control group difference in average earnings among female youths was not statistically significant at conventional levels. Nevertheless, the picture for female youths resembles in some ways the picture for adult women presented in Chapter 4, starting with an in-program period of foregone earnings, during which treatment group members earned less than control group members, followed by a post-program period during which the treatment group earned slightly more than the control group, at least initially. In the case of female youths, however, the return on their investment was not sustained in the way it was for adult women; treatment group earnings dropped below control group earnings toward the end of the follow-up period.

The trends in the control group's earnings also illustrate an important point: Even without JTPA the youths provided access to the program would have experienced substantial growth in monthly earnings. The estimates underlying the first graph (for all youths) indicate that this growth was from $313 in the first month to $523 in the eighteenth, or an increase of 67 percent. Among female youths this growth was from $223 in the first month to $399 in the eighteenth, or a 79 percent increase, while for male youths it was from $425 to $684, or a 61 percent increase.[8]

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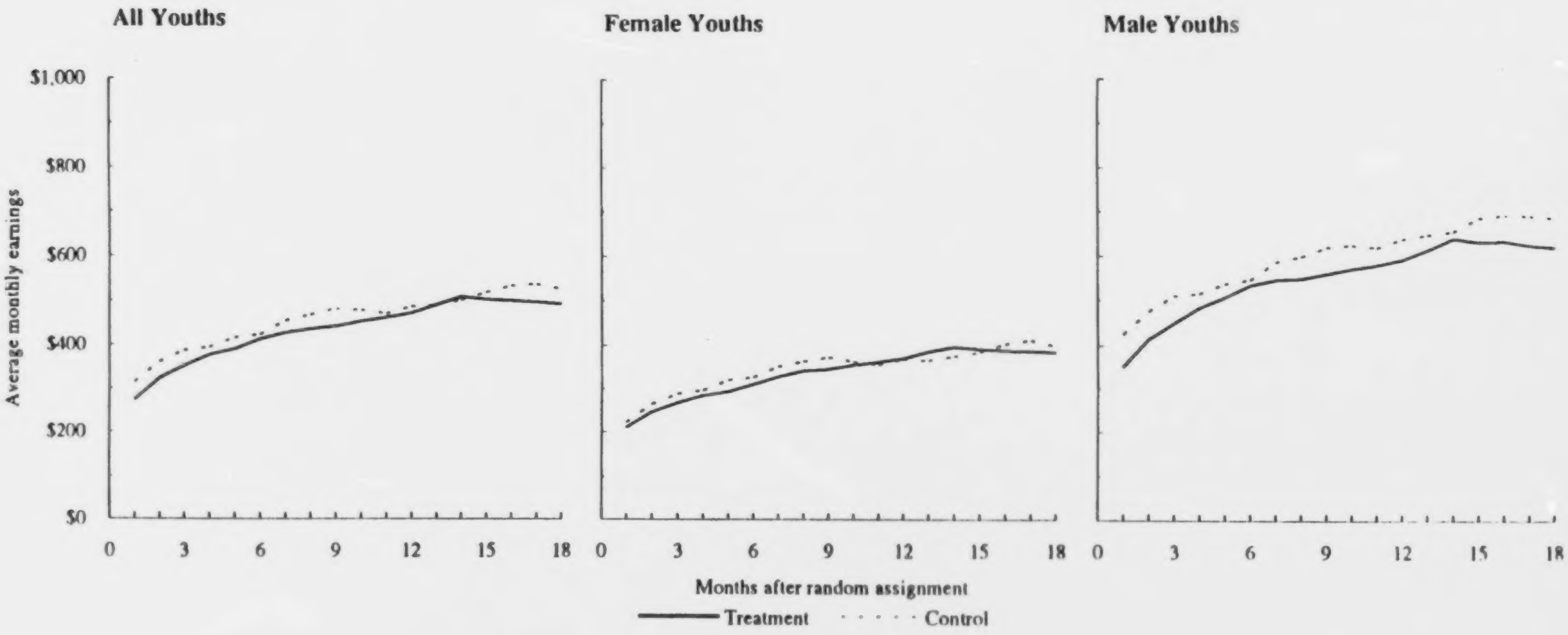
[7] Throughout this chapter, earnings and impact estimates are expressed in nominal dollars. The follow-up period varied across individuals, beginning as early as November 1987 and ending as late as December 1990.

[8] We used ordinary least squares regression procedures to increase the statistical precision of these estimates, as described in Appendix D. The earnings estimates in Exhibit 6.1 and subsequent exhibits include wages paid to JTPA participants in on-the-job training positions. During the 18-month follow-up period the program reimbursed employers a total of about $650 per *female youth OJT participant* and about $600 per *male youth OJT participant*. Among all youths in the sample OJT reimbursements over the follow-up period totaled about $60 per *female youth treatment group member* and about $80 per *male youth treatment group member*.

Exhibit 6.1 Average Monthly Earnings of Out-of-School Youths: Treatment Group and Control Group, Overall and by Gender



Source: Estimates based on First Follow-up Survey responses.
Notes: Sample size, all youths: treatment group = 2,782, control group = 1,266; female youths: treatment group = 1,586, control group = 714; male youths: treatment group = 1,196, control group = 552. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

A last point to note before leaving Exhibit 6.1 is the large difference between the earnings *levels* of females and males in every month during the follow-up period. The earnings of the male youth control group actually began in the first month at a higher level ($425) than the earnings of the female youth control group reached in the eighteenth month ($399). The large gender difference prevailed throughout the follow-up period, with the earnings of the male control group sometimes at a level almost twice that of the female control group.

ENROLLMENT PATTERNS OVER TIME: TREATMENT GROUP

As noted earlier, it is important to distinguish between the in-program and post-program periods for treatment group members, whose earnings may have been affected by forgone employment opportunities during the time they were enrolled in the program.

Recall that we are using the first month in which less than 15 percent of the treatment group was still enrolled in JTPA as the beginning of the *post-program period*. Among youth treatment group members overall this point was in month 8 (see Exhibit H.13 in Appendix H)—just beyond the start of the post-program period for adult men (month 7) and earlier than the starting point for adult women (month 10). For female youths the post-program period started in month 9 (Exhibit H.1), and for male youths, in month 7 (Exhibit H.7).[9]

As shown in Chapter 3 (Exhibit 3.20), the median *duration* of female youths' enrollment was more (at 3.9 months) than that of male youths (at 3.1 months). The corresponding figures for adults were 3.6 months for women and 2.5 months for men.

IMPACTS ON EARNINGS: JTPA ASSIGNEES AND ENROLLEES

Exhibit 6.2 quantifies the information in the last two graphs of Exhibit 6.1, aggregated into quarters and totals for the full 18 months.[10] As shown in the top panel, the estimated program impacts on the earnings of female youth assignees (treatment group members) were negative during the three predominantly in-program quarters, virtually zero for the

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[9] The enrollment rates reported in this paragraph are based on data from SDA records that somewhat overstate the number of persons still enrolled in the program at any given time because the data are missing some termination dates. Thus, Exhibit H.13 serves as an upper bound on the percentage of the treatment group still in the program in any given month; and the extent to which the graph overstates the actual enrollment rates is higher in the later months.

[10] A similar exhibit for all youths combined appears in Appendix H (Exhibit H.14), as do estimates of the effect of JTPA on the distribution of earnings for all youths (H.15), female youths (H.2), and male youths (H.8).

Exhibit 6.2 *Impacts on Earnings: JTPA Assignees and Enrollees, by Gender*

Period		*Control mean (1)*	*Impact per assignee*		*Inferred impact per enrollee, in $ (4)*
			In $ (2)	*As % of (1) (3)*	
			Female youths		
Quarter	1	$ 775	$ -49	-6.4%	$ -80
	2	943	-56	-5.9	-90
	3	1,084	-73	-6.8	-119
	4	1,084	2	0.2	3
	5	1,124	50	4.4	80
	6	1,214	-55	-4.5	-88
All quarters		6,225	-182	-2.9	-294
			Male youths		
Quarter	1	$ 1,412	$ -199**	-14.1%	$ -316
	2	1,598	-72	-4.5	-114
	3	1,803	-151*	-8.4	-240
	4	1,876	-138	-7.3	-219
	5	1,984	-105	-5.3	-167
	6	2,063	-189**	-9.2	-300
All quarters		10,736	-854**	-7.9	-1,356

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, female youths: assignees = 1,586, control group = 714; male youths: assignees = 1,196, control group = 552. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

fourth quarter, positive for the fifth, and then negative again during the sixth.[11] For the full 18-month follow-up period the estimated impact was $-182, or -2.9 percent of the control group mean of $6,225. None of these findings for female youths was statistically significant.

The bottom panel of the exhibit shows strikingly negative results for male youths, unlike the small and statistically insignificant findings for female youths. The estimated impacts on earnings were negative in all six quarters and statistically significant in three. Over the follow-up period as a whole the average program impact was a statistically significant $-854, or -7.9 percent of the control group mean. As discussed later in this chapter, however, the negative impact estimate for male youths overall reflects mainly a large, negative impact estimate for the subgroup of male youths who reported having been arrested between age 16 and their random assignment.[12] For those male youths who had not been arrested the estimated impact was small (\$-262) and not statistically significant. Thus, JTPA had similarly negligible impacts on most male youths as it did on female youths overall, and an extremely negative impact on the subgroup of male youths with a previous arrest.

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[11] To increase the statistical precision of these estimates, we used ordinary least squares regressions. This reduced the standard errors of the impact estimates but did not appreciably affect the point estimates, because the average values of the independent variables (mainly the baseline characteristics of the treatment and control groups) were virtually the same for the two groups. See Appendix D for a description of these procedures.

Column 1, for the control group, shows what the male youths who had access to JTPA would have earned without the program: \$10,736 over the 18 months—almost three-quarters more than the average for female youths. The control group experienced strong growth in earnings over time, from \$1,412 in the first quarter to \$2,063 in the sixth, or a 46 percent increase. The markedly negative and sustained impacts shown in column 2 imply that having access to JTPA indeed reduced male youths' earnings, on average, at least during these first 18 months of follow-up.

As shown in Exhibit 3.12 of Chapter 3, not all youths who were given access to JTPA actually became enrolled in the program; 65.5 percent of female youth assignees and 66.8 percent of male youth assignees were ultimately enrolled in the program during the follow-up period. Thus, as we did for adult women and men in the previous chapters, we also calculated inferred estimates of the program *impact per JTPA enrollee*.[13] As shown in column 4, the inferred impact per female youth enrollee was \$-294, and the inferred impact per male youth enrollee was \$-1,349. Again, because of the uncertainty about whether or not treatment group *nonenrollees* experienced some impact from their contact with JTPA (see Appendix F), we did not attempt to calculate the statistical significance of the inferred impacts per enrollee.

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[12] The extreme finding for this subgroup—a highly significant \$-3,038 earnings loss—based on the First Follow-up Survey data is not supported by the impact estimate obtained from an alternative data source, namely, earnings data from state unemployment insurance agencies (see Appendix E). These data show a negligible impact of \$34 on male youths with a previous arrest and a small (\$-240) negative impact on male youths overall, in contrast to the large, negative survey-based estimates. The UI-based estimates are derived from only four quarters of follow-up earnings and are also based on a smaller, nonrandom sample of the 18-month study sample. But neither data source produces results that suggest positive program impacts for male youths with or without a previous arrest. Impact findings from the two data sources for the other three target groups—adult women, adult men, and female youths—and for male youths without a previous arrest are quite similar, however (see Appendix E). In our forthcoming final report we will investigate further the discrepancy in estimates from these two data sources.

[13] As explained in Appendix D, the adjustment factor used to derive impacts per enrollee from impacts per assignee is $1/(r - c)$, where r is the enrollment rate (the proportion of treatment group members who were enrolled in JTPA) and c is the crossover rate (the proportion of control group members who were enrolled in JTPA). Since these two rates are fixed for a given group or subgroup, the ratio of impacts per enrollee to impacts per assignee is also fixed for that group or subgroup. Thus, for example, for the female youth target group the impact per enrollee is 1.62 (1/[.655 - .037]) times the impact per assignee for all outcomes in all time periods, while for the male youth target group the impact per enrollee is 1.59 ([.668 - .038]) times the impact per assignee.

IMPACTS ON EARNINGS: YOUTHS AND YOUNG ADULTS

The statistically insignificant $-182 earnings loss for female youths and the significant $-845 earnings loss for male youths differ substantially from the significant $539 earnings gain estimated for adult women and the near-significant $550 earnings gain estimated for adult men. One way to investigate possible sources of the different findings for youths is to examine whether there is an age-related pattern to the impacts.

For example, if the differences in impacts between adults and youths were determined by *age-related characteristics* per se, one would expect to see a pattern of *consistently less positive impacts* in looking from older young adults on down in age to the youngest youths in the sample. On the other hand, if adults and youths were *recruited* or *served* differently by JTPA Title II-A (and thus either participants or the program services differed substantially), one would expect to see an *abrupt shift in impacts,* from positive effects for young adults to negative effects for youths—at precisely the demarcation that defines the two groups, namely, between ages 22 and 21.

Exhibit 6.3 allows us to investigate these possibilities by presenting separate impact estimates for each two-year age cohort of young adults (ages 22 to 29) and youths (ages 16 to 21). The findings appear to indicate more of an abrupt shift from positive to negative impacts at the demarcation between adults and youths than a pattern of gradually diminishing impacts over the entire age range. Tests of statistical significance indicated, however, that the deviation of the impacts for youths from the *trend* in impacts for young adults was not statistically significant.[14] Thus, the findings in Exhibit 6.3 are inconclusive. Nevertheless, they at least suggest the possibility of some fundamental differences in the types of adults and out-of-school youths recruited to Title II-A or in the types of services the program delivered to these groups, or in both program recruits and program services.

Impacts on Earnings and Its Components: Female Youths Recommended for Each Service Strategy

The impact estimates presented above are averages for all female and male out-of-school youths in the 18-month study sample. They therefore reflect the effects of JTPA Title II-A on a variety of different individuals whom program staff recommended for a number

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[14] As shown in Exhibit H.22 in Appendix H, the estimated deviations from the young adult impact trend were $-169 for female youths ages 20 to 21 and $-702 for male youths ages 20 to 21. Estimates of the deviations from the young adult trend for youths ages 16 to 17 and youths ages 18 to 19 were negative at a much greater magnitude ($-1,912 and $-2,118, respectively) but still statistically insignificant for males, and positive ($933 and $211, respectively) though again statistically insignificant for females.

Exhibit 6.3 *Impacts on Total 18-Month Earnings: JTPA Assignees Ages 16-29, by Gender and Two-Year Age Group*

Age group	*Sample size*[a] *(1)*	*Mean earnings* *Assignees* *(2)*	*Mean earnings* *Controls* *(3)*	*Impact per assignee* *In $* *(4)*	*Impact per assignee* *As % of (3)* *(5)*
			Females		
16 - 17	360	$ 3,748	$ 3,643	$ 105	2.9%
18 - 19	1,040	6,153	6,437	-284	-4.4
20 - 21	900	6,763	7,157	-394	-5.5
22 - 23	724	8,168	7,595	573	7.5
24 - 25	740	7,896	7,951	-55	-0.7
26 - 27	713	7,998	7,327	671	9.2
28 - 29	653	7,939	6,565	1,374**	20.9
			Males		
16 - 17	299	$ 9,977	$ 10,772	$ -795	-7.4%
18 - 19	754	9,407	10,776	-1,369**	-12.7
20 - 21	695	11,598	12,017	-419	-3.5
22 - 23	584	13,805	13,176	629	4.8
24 - 25	502	13,358	13,211	148	1.1
26 - 27	438	13,106	12,360	746	6.0
28 - 29	431	13,983	13,499	484	3.6

Sources: Estimates based on First Follow-up Survey responses. The estimates for women ages 22-29 at random assignment are also based on earnings data from state unemployment insurance (UI) agencies.

Notes: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. Treatment and control groups combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 5 are identical to those in column 4.

of different employment and training services. This section examines the subgroups of female youths recommended for each of the three service strategies, presenting, in turn, findings on service receipt, baseline characteristics, and finally labor market impacts for each subgroup.

Before turning to these findings, however, note that the service strategies that SDA staff recommended for youths differed from those they recommended for adults. Service strategy recommendations also varied by gender.

As shown in Exhibit 3.16 in Chapter 3, youths were more apt than adults to be recommended for the other services strategy—and less apt to be recommended for the OJT/JSA strategy. In the case of females the other services strategy was recommended for 32.5 percent of the youths but only 21.0 percent of the adults; whereas the OJT/JSA strategy was deemed appropriate for only 23.2 percent of the youths but 35.0 percent of the adults. Female youths and adult women were recommended for the classroom training strategy, however, in the same proportion: roughly 44 percent. And as with the two adult target groups, the main differences between the service recommendations for female and male youths were that the females were more likely to be recommended for classroom training, whereas the males were more likely to be recommended for OJT/JSA.

DIFFERENCES IN EMPLOYMENT AND TRAINING SERVICES RECEIVED: TREATMENT AND CONTROL GROUPS WITHIN EACH SERVICE STRATEGY SUBGROUP

As explained in Chapter 2, we define the impact estimates in this report as representing the *incremental* effect of the difference between the mix of services received by the treatment group and the mix received by the control group. Exhibit 6.4 allows us to assess the size of this increment by comparing the percentages of treatment and control group members in each service strategy subgroup who received each specific program service (our measure of the *likelihood* of receiving the service) and the mean number of hours of each service received (the *amount* of the service received). The exhibit also allows us to assess differences in the types of services received across subgroups.

The Classroom Training Subgroup. Although all members of the classroom training subgroup were recommended for classroom training in occupational skills, only 48.3 percent of the female youth treatment group in this subgroup reported receiving that service, as shown in the first panel of the exhibit.[15] The principal reason why some female youths did not receive that recommended service was that 28.5 percent of the treatment group in this subgroup were never enrolled in JTPA during the follow-up period (Exhibit 3.12 in Chapter 3).

The estimates of service receipt nevertheless show that JTPA did increase the likelihood of receiving the two key services in this service strategy. Specifically, treatment group members were more likely than control group members to receive classroom training in occupational skills (48.3 percent versus 31.0 percent) and to receive basic education (16.5 percent versus 12.2 percent).

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[15] The survey-based estimates of receipt of classroom training in occupational skills and basic education shown in Exhibit 6.4 differ from the estimates based on SDA data in Chapter 3 (Exhibit 3.18), for two reasons. First, the survey did not attempt to distinguish between services provided by JTPA and non-JTPA providers, whereas the SDA data cover JTPA-funded services only. Second, the survey-based estimates are subject to respondent error in recalling and classifying services. In general, the SDA data are more reliable measures of *JTPA* services received. The survey-based estimates, however, are the only available measures of service receipt that include non-JTPA services and that are therefore comparable for the treatment and control groups.

Exhibit 6.4 *Receipt of Employment and Training Services: Female Youth Treatment Group and Control Group, by Service Strategy Subgroup*

	Percentage receiving service			*Mean hours of service per sample member*		
Specific program service	*Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Treatment group (4)*	*Control group (5)*	*Difference, in hours (6)*
			Classroom training subgroup			
Classroom training in occupational skills[a] +	48.3%	31.0%	17.3%	378	191	187
Basic education[b] +	16.5	12.2	4.3	73	37	36
On-the-job training ++ (JTPA only)	2.6	0.0	2.6	24 [c]	0 [c]	24 [c]
Work experience ++ (JTPA only)	5.7	1.0	4.7	33 [c]	1 [c]	32 [c]
Job search assistance +++ (JTPA only)	27.3	–	–	–	–	–
Miscellaneous [d] +++ (JTPA only)	7.7	–	–	–	–	–
			OJT/JSA subgroup			
Classroom training in occupational skills[a] +	17.9%	17.6%	0.3%	101	117	-16
Basic education[b] +	5.4	7.9	-2.5	20	22	-2
On-the-job training ++ (JTPA only)	29.9	0.5	29.4	111 [c]	6 [c]	105 [c]
Work experience ++ (JTPA only)	5.2	1.6	3.7	20 [c]	3 [c]	17 [c]
Job search assistance +++ (JTPA only)	28.3	–	–	–	–	–
Miscellaneous [d] +++ (JTPA only)	7.1	–	–	–	–	–

(Continued)

Exhibit 6.4 Receipt of Employment and Training Services: Female Youth Treatment Group and Control Group, by Service Strategy Subgroup (continued)

Specific program service	*Percentage receiving service* *Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Mean hours of service per sample member* *Treatment group (4)*	*Control group (5)*	*Difference, in hours (6)*
			Other services subgroup			
Classroom training in occupational skills[a] +	24.9%	23.9%	1.0%	133	132	1
Basic education[b] +	23.1	19.1	4.0	70	67	3
On-the-job training ++ (JTPA only)	3.9	0.4	3.5	22[c]	1[c]	21[c]
Work experience ++ (JTPA only)	3.4	2.0	1.4	19[c]	16[c]	3[c]
Job search assistance+++ (JTPA only)	12.2	–	–	–	–	–
Miscellaneous[d] +++ (JTPA only)	28.5	–	–	–	–	–

Sources:

+ Unadjusted frequencies in this row are based on First Follow-up Survey data on receipt of the service from any provider.

++ Unadjusted frequencies in this row are based on enrollment and tracking data from the 16 SDAs, the best available data on receipt of this service. Although the data are for JTPA Title II-A-funded services only, this service is typically not funded by non-JTPA providers.

+++ Unadjusted frequencies in this row are also based on SDA enrollment and tracking data. Comparable data on receipt of this service from other providers were not available; nor were comparable data on receipt by control group members.

Notes: Sample size, classroom training subgroup: treatment group = 704, control group = 341; OJT/JSA subgroup: treatment group = 381, control group = 164; other services subgroup: treatment group = 501, control group = 209. Because of missing data, sample sizes for services calculated from different data sources may vary. Tests of statistical significance were not performed for this exhibit.

a. Lasting longer than one week.

b. Lasting longer than one week. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. Hours, assuming a full-time job at 40 hours per week.

d. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

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The treatment-control group differences in the amount of each service received were 187 additional hours of occupational skills training and 36 additional hours of basic education per treatment group member.[16] These differentials reflect differences both in the *likelihood* of receiving the two services and in the *amount* of the service received *per recipient*. Finally, note that job search assistance was also a commonly received service in the classroom training subgroup, probably as an adjunct to classroom services.

The OJT/JSA Subgroup. As noted in chapters 4 and 5, we were unable to measure receipt of on-the-job training from non-JTPA providers. Nonetheless, the treatment-control group difference in receipt of OJT from JTPA is probably a reasonably reliable indicator of the overall difference, since few non-JTPA providers offer this service. As shown in the second panel of Exhibit 6.4, among female youths recommended for the OJT/JSA strategy, only 29.9 percent of the treatment group received OJT, the defining service for the strategy. Again, the main reason for nonreceipt was a failure to enroll in JTPA: 42.5 percent of the treatment group in this subgroup were not enrolled (Exhibit 3.12 in Chapter 3).

Merely 0.5 percent of the control group received OJT, however. Consequently, JTPA provided an additional 105 hours of OJT receipt, averaged over all treatment group members. Exhibit 6.4 shows that 28.3 percent of the OJT/JSA treatment group received job search assistance, the second key service in the OJT/JSA strategy. But because we could not measure the control group's receipt of this service, we could not determine the corresponding treatment-control group difference.

The Other Services Subgroup. We were also unable to measure receipt of miscellaneous services by the control group, and so we could not estimate the treatment-control group difference in receipt of this category of services, which was the most common category received by female youth treatment group members in the other services subgroup. We do know, however, that nearly a quarter of the female youth treatment group received classroom training in occupational skills and nearly a quarter received basic education, but treatment-control group differences in receipt of these services were small. The exhibit also shows that job search assistance was not a prominent service received by female youths in this subgroup.

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[16] The hours shown in columns 4 through 6 are averages over all sample members, including those who were not enrolled in the service. To calculate the average number of hours of service receipt for only those sample members who actually received the service, divide the number of hours of service receipt per treatment group member *or* control group member (column 4 *or* 5) by the corresponding proportion receiving the service (column 1 *or* 2, divided by 100).

Summary of Differences in Service Receipt. The preceding comparisons of service receipt across service strategy subgroups and between the treatment and control groups within each subgroup can be summarized as follows:

- Many female youth treatment group members did not receive the service recommended for them, either because they were never enrolled in JTPA or because they received a service other than the primary one recommended for them.

- Nevertheless, the three service strategy subgroups represent distinctly different clusters of services actually received and are therefore useful for analyzing the effects of alternative service strategies.

- Within each service strategy subgroup most female youths who were enrolled in JTPA received one or both of two key services, which varied by service strategy: In the classroom training subgroup these were classroom training in occupational skills and basic education; in the OJT/JSA subgroup they were on-the-job training and job search assistance; and in the other services subgroup they were miscellaneous services and basic education.

- The services actually received by each service strategy subgroup were consistent with the recommendations of intake staff to a considerable extent, but they differed in some important ways. Most important was the finding that only 29.9 percent of female youth treatment group members recommended for OJT (the OJT/JSA subgroup) actually received that service. Because 28.3 percent of female youth treatment group members recommended for OJT received job search assistance, it is most appropriate to characterize the OJT/JSA strategy as one based on placement in employment, with or without subsidized training.

- Treatment-control group differences in the average *amount* of service received were relatively modest, where those differences could be measured. The average amount of additional occupational skills training received by treatment group members in the classroom training subgroup was 187 hours. Similarly, the treatment-control group difference in receipt of OJT by female youths in the OJT/JSA subgroup was only 105 hours. These modest service differentials reflect the fact that not all treatment group members received these services and, in the case of occupational skills training, that some control group members received the service.

Exhibit 6.5 *Impacts on Attainment of a Training-Related High School Diploma or GED Certificate: Female Youth JTPA Assignees Overall and High School Dropout Subgroup, by Service Strategy Subgroup*

		Percentage attaining a training-related high school credential[b]		
	Sample size[a] *(1)*	*Assignees (2)*	*Control group (3)*	*Difference, in % pts. (4)*
		Classroom training subgroup		
Full sample	1,002	15.4%	7.6%	7.8%***
High school dropouts	467	32.9	16.6	16.4***
		OJT/JSA subgroup		
Full sample	516	3.4	1.9	1.5
High school dropouts	172	9.8	6.0	3.8
		Other services subgroup		
Full sample	679	19.0	13.1	5.9**
High school dropouts	411	31.7	21.0	10.7**
		All subgroups		
Full sample	2,197	13.7	7.9	5.8***
High school dropouts	1,050	28.6	16.6	11.9***

Sources: Unadjusted frequencies based on Background Information Form responses and First Follow-up Survey responses.

a. Treatment and control groups combined.

b. "Attainment of a training-related high school credential" is defined as the combination of having received some school or training service and having attained a high school diploma or General Education Development Certificate at some time during the 18-month follow-up period.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Impacts on High School Attainment. As part of the training services reviewed above, a number of female youths gained a high school diploma or GED certificate. Exhibit 6.5 displays program impacts on the *attainment of a training-related high school credential,* which we define as the percentage of the sample who reported both participating in a school or training program and attaining a high school credential at some time during the 18-month follow-up period. The exhibit reports impacts for both the sample overall and high school dropouts only.

As can be seen, JTPA led to statistically significant gains in high school attainment for female youth assignees overall and for the dropouts, in both the classroom training and the other services subgroups—the two subgroups with relatively high rates of receiving basic education. These results were reflected in a significant 5.8 percentage point increase for assignees overall and a significant 11.9 percentage point increase for dropouts overall.

DIFFERENCES IN BASELINE CHARACTERISTICS ACROSS SERVICE STRATEGY SUBGROUPS: JTPA ASSIGNEES

As explained in Chapter 2, variations in impacts across service strategy subgroups reflect not only the difference in the services each subgroup received, but also differences in the individuals recommended for each strategy.

Columns 2 through 4 of Exhibit 6.6 show why it is not possible to use the estimated impacts by service strategy subgroup to determine the best service strategy for the *average* female out-of-school youth in JTPA: because members of each of the three service strategy subgroups were quite different from those in the other subgroups.

By almost every measure the *OJT/JSA subgroup* appeared to be the most employable and the *other services subgroup* to be the least employable, with the *classroom training subgroup* in-between. For example, the OJT/JSA subgroup was the most apt to have a high school credential upon application (64.5 percent), to have worked before (87.4 percent), and to be over age 19 (46.3 percent). The other services subgroup, on the other hand, was the most likely to have worked less than 13 weeks in the past 12 months (29.8 percent), to be receiving public assistance (54.2 percent), and to be living in a family with an annual income of less than $3,000 (52.4 percent). These differences were also reflected in the mean earnings of each subgroup over the year preceding application: $1,795 for the OJT/JSA subgroup, $1,286 for the classroom training subgroup, and only $996 for the other services subgroup.[17]

IMPACTS ON EARNINGS: JTPA ASSIGNEES AND ENROLLEES IN EACH SERVICE STRATEGY SUBGROUP

As we have seen, both the characteristics of sample members and the services they received differed among the three service strategy subgroups. Moreover, chapters 4 and 5 found different patterns of impacts for sample members deemed appropriate for the different service strategies. Separating the sample in this way may therefore provide some insight into the

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17. Exhibit 3.17 in Chapter 3, which displays mean earnings of the control group over the 18-month follow-up period, shows similar differences among the three subgroups. For a more detailed description of the baseline characteristics of these subgroups, see Bloom (1991). Note, however, that the data in Bloom (1991) cover all JTPA applicants randomly assigned to treatment or control status, whereas Exhibit 6.6 includes only the female youth treatment group in the smaller 18-month study sample, Appendix A in the present report compares the treatment and control groups in this sample.

Exhibit 6.6 Selected Baseline Characteristics: Female Youth JTPA Assignees, by Service Strategy Subgroup

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
Ethnicity				
White, non-Hispanic	50.1%	51.1%	59.9%	41.7%
Black, non-Hispanic	32.5	24.8	25.2	48.1
Hispanic	15.8	22.4	13.1	8.8
American Indian or Alaskan Native	0.8	0.5	1.4	0.8
Asian or Pacific Islander	0.8	1.2	0.5	0.5
Barriers to employment				
Receiving cash welfare[a]	30.3%	34.4%	23.1%	29.8%
No high school diploma or GED certificate	49.2	48.2	35.5	60.0
Worked less than 13 weeks in past 12 months	60.5	60.4	46.4	70.6
Number of barriers				
None of the above	20.8	20.2	33.6	12.6
One of the above	32.3	32.2	35.9	30.0
Two of the above	32.3	32.5	22.7	38.8
All three of the above	14.6	15.0	7.8	18.6
Work and training histories				
Ever employed	76.9%	75.0%	87.4%	72.0%
Mean individual earnings in past 12 months	$1,305	$1,286	$1,795	$996
Hourly earnings in last job				
Never employed	23.1%	25.0%	12.6%	28.0%
Less than $4	47.6	47.7	51.2	44.8
$4 or more	29.3	27.3	36.2	27.2
Employed upon application	14.4	16.2	16.9	10.3
Previously received occupational training	25.5	21.5	33.4	25.3
Public assistance status				
Receiving any public assistance[b]	47.1%	48.0%	40.5%	50.7%
Receiving AFDC	26.6	30.7	18.1	27.0
Receiving food stamps	39.3	38.9	31.6	45.6
Receiving other public assistance[c]	10.5	10.9	10.0	10.4

(Continued)

Exhibit 6.6 Selected Baseline Characteristics: Female Youth JTPA Assignees, by Service Strategy Subgroup (continued)

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
AFDC history				
Never AFDC case head	71.0%	67.8%	75.7%	71.9%
AFDC case head less than 2 years	21.9	23.7	19.1	21.3
AFDC case head 2 years or more	7.2	8.5	5.2	6.8
JTPA required for welfare, food stamps, or WIN program [d]	7.3%	6.6%	3.9%	10.7%
Household composition				
No spouse or own child present	52.5%	52.0%	52.0%	57.7%
Own child any age, no spouse present	36.2	33.5	35.9	34.2
Spouse present, with or without own child	11.4	14.5	12.1	8.1
Family income in past 12 months				
< $3,000	45.8%	43.9%	40.5%	52.2%
$3,000 - $6,000	28.5	30.6	27.3	26.5
$6,001 - $9,000	9.8	9.9	10.6	9.2
> $9,000	15.9	15.6	21.6	12.2
Living in public housing				
Yes	13.9%	12.6%	11.4%	17.3%
No	86.1	87.4	88.6	82.7
Age at random assignment				
16 - 19	59.4%	57.9%	53.7%	65.4%
20 - 21	40.6	42.1	46.3	34.6
Mean	*19.0*	*19.0*	*19.4*	*18.8*
Arrested since age 16 [e]				
Yes	6.1%	5.8%	8.5%	4.7%
No	93.9	94.2	91.5	95.3
Sample size	*1,814*	*803*	*421*	*590*

Source: Unadjusted frequencies based on Background Information Form responses.

a. AFDC, General Assistance, or other welfare except food stamps.

b. "Any public assistance" includes the following sources of assistance: AFDC, food stamps, unemployment insurance, housing assistance, and other cash assistance.

c. "Other public assistance" includes unemployment insurance, housing assistance, and other (non-AFDC) cash assistance.

d. WIN is the federal Work Incentive program.

e. The percentage arrested between age 16 and random assignment is based on responses to the First Follow-up Survey by treatment group members who were included in the analysis of impacts on earnings.

impact estimates for female youths overall. We do so in Exhibit 6.7.[18] Although only a few of the estimates shown were statistically significant, the exhibit suggests some variation in impacts across the service strategy subgroups.

More specifically, the *classroom training strategy* yielded large, statistically significant earnings losses during the first three, predominately in-program quarters, followed by small and statistically insignificant gains for the next two quarters, and then a small and insignificant loss in the last quarter. Over the period as a whole the classroom training subgroup had an earnings loss of $-542, or -9.1 percent of the control group mean of $5,936, although this estimate was not statistically significant.

The *OJT/JSA strategy*, on the other hand, should not delay employment, and the exhibit suggests that no such delay occurred for the subgroup of female youths recommended for this strategy. Impacts were positive for the first five quarters but statistically significant only in quarter 2. Over the 18 months of follow-up the OJT/JSA subgroup gained $410, or 5.4 percent of the control mean of $7,620, but again this estimate was not statistically significant.[19]

The *other services strategy* yielded an estimated impact of $-158, or 2.8 percent of the control mean of $5,726, over the 18 month follow-up period. The estimated impacts were small and negative in four of the six quarters but none was statistically significant.

IMPACTS ON THE COMPONENTS OF EARNINGS: JTPA ASSIGNEES IN EACH SERVICE STRATEGY SUBGROUP

Exhibit 6.8 presents the findings of our decomposition of the percentage impact on earnings for female youths into its four components: *workers per assignee, weeks worked per worker, hours worked per week worked,* and *earnings per hour worked* (columns 2 through 5). As explained in Chapter 4, each of these components reflects a different aspect of labor market success: the ability to find a job; how quickly assignees found jobs and how long they held them; the extent of full-time work; and how much workers were paid for the time they worked. Again, the estimates in columns 3 through 5 of the exhibit apply to only those sample members who were employed during the period in question. And the four

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[18] Graphs of the monthly earnings trends on which Exhibit 6.7 is based appear as Exhibit H.5 in Appendix H. As noted earlier, the earnings and impact estimates include wages paid to JTPA participants in OJT positions. The program reimbursed employers about $650 per female youth *OJT participant* during the 18-month follow-up period. Within the OJT/JSA subgroup the reimbursement was about $160 per female youth *assignee* over the follow-up period.

[19] As noted earlier, the earnings estimates include wages paid to sample members placed in OJT positions.

Exhibit 6.7 *Impacts on Earnings: Female Youth JTPA Assignees and Enrollees, by Service Strategy Subgroup*

Period	*Control mean (1)*	*Impact per assignee*		*Inferred impact per enrollee, in $ (4)*
		In $ (2)	*As % of (1) (3)*	
		Classroom training subgroup		
Quarter 1	$ 742	$ -210***	-28.3%	$ -307
2	909	-189***	-20.8	-276
3	1,052	-150*	-14.2	-219
4	991	24	2.4	35
5	1,047	70	6.7	102
6	1,196	-87	-7.3	-127
All quarters	5,936	-542	-9.1	-792
		OJT/JSA subgroup		
Quarter 1	$ 1,002	$ 149	14.9%	$ 277
2	1,074	203*	18.9	377
3	1,252	97	7.7	180
4	1,363	3	0.2	6
5	1,368	103	7.6	191
6	1,562	-146	-9.3	-271
All quarters	7,620	410	5.4	762
		Other services subgroup		
Quarter 1	$ 653	$ 43	6.5%	$ 74
2	909	-68	-7.4	-117
3	1,023	-96	-9.4	-165
4	1,047	-52	-5.0	-89
5	1,093	-41	-3.8	-70
6	1,001	55	5.6	94
All quarters	5,726	-158	-2.8	-271

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 704, control group = 341; OJT/JSA subgroup: assignees = 381, control group = 164; other services subgroup: assignees = 501, control group = 209. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

Exhibit 6.8 Percentage Impacts on Earnings and Its Components: Female Youth JTPA Assignees, by Service Strategy Subgroup

Period	*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
			Classroom training subgroup		
Quarter 1	-28.3%	-19.7%	-7.8%	-2.8%	-0.4%
2	-20.8	-13.1	-7.9	-2.6	1.7
3	-14.2	2.8	-10.8	-5.7	-0.8
4	2.4	8.1	-3.5	-3.9	2.0
5	6.7	1.7	1.6	0.3	3.0
6	-7.3	1.2	-3.0	-1.9	-3.8
All quarters	-9.1	4.7	-11.5	-2.6	0.6
			OJT/JSA subgroup		
Quarter 1	14.9%	11.2%	-5.1%	1.4%	7.2%
2	18.9	10.3	-4.7	3.6	9.1
3	7.7	2.2	-2.7	2.8	5.2
4	0.2	-1.9	-4.5	4.0	2.9
5	7.6	-5.2	1.3	3.4	8.3
6	-9.3	-9.0	-7.7	0.8	7.0
All quarters	5.4	4.3	-7.3	2.7	6.2
			Other services subgroup		
Quarter 1	6.5%	19.3%	-8.9%	-1.1%	-1.0%
2	-7.4	-4.8	-5.3	4.4	-1.8
3	-9.4	-6.9	0.3	-2.2	-0.9
4	-5.0	-1.8	2.9	1.8	-7.7
5	-3.8	1.2	-1.5	0.8	-4.2
6	5.6	1.0	0.3	2.2	1.7
All quarters	-2.8	3.4	-4.5	0.9	-2.4

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 704, control group = 341; OJT/JSA subgroup: assignees = 381, control group = 164; other services subgroup: assignees = 501, control group = 209. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is calculated as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

components are related to the estimated impact on earnings in column 1 by a simple multiplicative identity, which means that in each row of the exhibit the estimates in columns 2 through 5 sum to roughly equal the estimate in column 1.[20]

As shown in the top panel, in the *classroom training subgroup* the percentage impact on the employment rate (column 2) was clearly negative during the first two (in-program) quarters, then appeared to turn positive or near zero in succeeding quarters. The estimated impacts on weeks worked per worker and hours worked per week worked were negative in every quarter but one. For all quarters combined the strongest negative factor in the -9.1 percent earnings loss was the -11.5 percent reduction in weeks worked per worker. Thus, the negative impact on earnings in this subgroup appears to reflect primarily a reduction in how long sample members held jobs, rather than whether they found a job at all, how many hours they worked per week, or how much they were paid for the jobs they held.

In the *OJT/JSA subgroup* the pattern was somewhat different. During the first two quarters more treatment group than control group members worked, as JTPA placed some treatment group members in OJT positions or regular jobs. That soon ended, however, and employment rates in the control group exceeded those in the treatment group during the last three quarters. Impacts on weeks worked per worker and hours worked per week worked present a somewhat contradictory story. Assignees who worked tended to work fewer weeks but more hours per week than control group members. The impacts on earnings per hour worked do show a pattern, however: consistently higher earnings among treatment group members who worked than among control group members who worked. In sum, the initial boost in employment from the OJT/JSA strategy was gradually reversed, but treatment group members who worked ended up in better paying jobs on average but for shorter periods of time, yielding a 5.4 percent gain in earnings per assignee.

It is important to remember that, as explained in chapters 4 and 5, the estimated impacts on earnings per hour worked may reflect program effects on the composition of the subgroup of female youths who were employed, in addition to, or instead of, any impact on the hourly earnings of specific individuals. In other words, the estimates in column 5 do not necessarily imply that JTPA increased or decreased the *wage rates* of individual workers.[21]

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[20] For corresponding estimated impacts on female youths overall, see Exhibit H.3 in Appendix H. Exhibit H.3 in Appendix H presents impacts on the percentage employed and the number of weeks and hours worked for all female youths, including those who were never employed during the follow-up period.

[21] See Appendix G for a nonexperimental analysis, which showed no statistically significant effects on the latent wage rates of female youths.

In the *other service subgroup* the patterns were more complex, and no clear story emerges, as might be expected from a strategy with such a broad mix of services. For the 18-month follow-up period overall the negative impacts on weeks worked and hourly earnings per worker more than offset the positive impact on the employment rate, yielding a -2.8 percent loss in earnings per assignee.

Impacts on Earnings and Its Components: Male Youths Recommended for Each Service Strategy

This section examines the service receipt, baseline characteristics, and program impact findings for male youths in each of the three service strategy subgroups. The section will demonstrate that although the mix and amount of services received by male youths in each service strategy were similar to those received by their female youth counterparts, the males differed from the females in baseline characteristics, especially in their arrest rates and rates of single parenting. Moreover, the estimated program impacts by service strategy subgroup also differed markedly by gender.

DIFFERENCES IN EMPLOYMENT AND TRAINING SERVICES RECEIVED: TREATMENT AND CONTROL GROUPS WITHIN EACH SERVICE STRATEGY SUBGROUP

Before turning to the findings on service receipt by male youths, recall that youths were more likely than adults to be recommended for the other services strategy and less likely to be recommended for the OJT/JSA strategy. And as with the adult target groups, the male youths were more apt than the females to be recommended for OJT/JSA and less apt to be recommended for the classroom training strategy (see Exhibit 3.16, Chapter 3).

The Classroom Training Subgroup. As shown in Exhibit 6.9, 42.6 percent of male youth treatment group members in the classroom training subgroup reported receiving classroom training in occupational skills, whereas only 22.4 percent of their control group counterparts did. Again, the principal reason why some treatment group members did not receive this defining service for the strategy was that 25.2 percent of the treatment group in this subgroup were never enrolled in JTPA (Exhibit 3.12 in Chapter 3). Here the treatment-control group differential in the *likelihood* of receiving this service was similar to that for female youths. The differential in the case of basic education, however, was negligible for male youths (16.2 percent versus 15.4 percent), whereas it was small but noticeable for female youths.

The treatment group in this subgroup of male youths generally received a comparable number of hours of each service as their female youth counterparts, but the male youth control group in each case received a greater amount (number of hours) of each service. As a result, the treatment-control group differences in the amounts of each service received (column 6) were somewhat smaller for male youths than for female youths.

Note, also, that job search assistance was again a prominent service received by this subgroup, with 30.8 percent of male youth treatment group members having received it.

The OJT/JSA Subgroup. The male youth OJT/JSA subgroup was similar to the female youth OJT/JSA subgroup in the treatment group's likelihood of receiving on-the-job training, in the amount of OJT received, and in the treatment-control group differentials in both the likelihood and the amount of OJT receipt. Male youth treatment group members in this subgroup were somewhat more likely than their female youth counterparts to receive job search assistance (32.2 percent versus 28.3 percent), the other key service in this strategy.

The Other Services Subgroup. In this subgroup we find some differences in the types of services received by male and female youths. Specifically, male youth treatment group members in the other services subgroup were somewhat more likely than their female counterparts to receive miscellaneous services (35.3 percent versus 28.5 percent) and less likely to receive basic education (13.5 percent versus 24.9 percent).[22] Nevertheless, the males' relatively small likelihood of receiving job search assistance as part of this service strategy was virtually the same as the females. And again the treatment-control group differences in the likelihood and amount of receipt of classroom training in occupational skills and basic education were small.

Nevertheless, the males' relatively small likelihood of receiving job search assistance as part of this service strategy was virtually the same as the females'. And again, the treatment-control group differences in the likelihood and amount of receipt of classroom occupational skills training and basic education were small.

Summary of Differences in Service Receipt. The preceding comparisons of service receipt across service strategies and between the treatment and control groups within each service strategy subgroup have yielded basic findings quite similar to those for female youths.

Specifically, many treatment group members did not receive the service recommended for them, because of nonenrollment or receipt of another service. The three service strategy

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[22] Again, the survey-based estimates of receipt of classroom training in occupational skills and basic education shown in exhibits 6.4 and 6.9 differ from those based on SDA data in Chapter 3 (Exhibit 3.18), for reasons discussed earlier.

Exhibit 6.9 Receipt of Employment and Training Services: Male Youth Treatment Group and Control Group, by Service Strategy Subgroup

	Percentage receiving service			Mean hours of service per sample member		
Specific program service	*Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Treatment group (4)*	*Control group (5)*	*Difference, in hours (6)*
			Classroom training subgroup			
Classroom training in occupational skills[a] +	42.6%	22.4%	20.4%	321	193	127
Basic education[b] +	16.2	15.4	0.8	87	70	16
On-the-job training ++ (JTPA only)	4.4	0.9	3.5	20[c]	2[c]	18[c]
Work experience ++ (JTPA only)	6.5	1.9	4.6	39[c]	13[c]	25[c]
Job search assistance +++ (JTPA only)	30.8	--	--	--	--	--
Miscellaneous[d] +++ (JTPA only)	7.9	--	--	--	--	--
			OJT/JSA subgroup			
Classroom training in occupational skills[a] +	15.6%	8.8%	6.8%	106	48	58
Basic education[b] +	5.6	6.4	-0.8	9	41	-32
On-the-job training ++ (JTPA only)	30.5	0.8	29.7	131[c]	3[c]	128[c]
Work experience ++ (JTPA only)	4.2	1.6	2.6	16[c]	6[c]	10[c]
Job search assistance +++ (JTPA only)	32.2	--	--	--	--	--
Miscellaneous[d] +++ (JTPA only)	6.8	--	--	--	--	--

(Continued)

Exhibit 6.9 Receipt of Employment and Training Services: Male Youth Treatment Group and Control Group, by Service Strategy Subgroup (continued)

	Percentage receiving service			*Mean hours of service per sample member*		
Specific program service	*Treatment group (1)*	*Control group (2)*	*Difference, in % pts. (3)*	*Treatment group (4)*	*Control group (5)*	*Difference, in hours (6)*
			Other services subgroup			
Classroom training in occupational skills [a]+	21.3%	20.1%	1.2%	129	119	10
Basic education [b]+	13.5	12.4	1.1	85	86	-1
On-the-job training ++ (JTPA only)	3.9	0.0	3.9	14[c]	0[c]	14[c]
Work experience ++ (JTPA only)	3.2	0.8	2.4	22[c]	14[c]	8[c]
Job search assistance +++ (JTPA only)	12.0	–	–	–	–	--
Miscellaneous [d]+++ (JTPA only)	35.3	–	–	–	–	--

Sources:

+ Unadjusted frequencies in this row are based on First Follow-up Survey data on receipt of the service from any provider.

++ Unadjusted frequencies in this row are based on enrollment and tracking data from the 16 SDAs, the best available data on receipt of this service. Although the data are for JTPA Title II-A-funded services only, this service is typically not funded by non-JTPA providers.

+++ Unadjusted frequencies in this row are also based on SDA enrollment and tracking data. Comparable data on receipt of this service from other providers were not available; nor were comparable data on receipt by control group members.

Notes: Sample size, classroom training subgroup: treatment group = 354, control group = 172; OJT/JSA subgroup: treatment group = 411, control group = 204; other services subgroup: treatment group = 431, control group = 176. Because of missing data, sample sizes for services calculated from different data sources may vary. Tests of statistical significance were not performed for this exhibit.

a. Lasting longer than one week.

b. Lasting longer than one week. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. Hours, assuming a full-time job at 40 hours per week.

d. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

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subgroups do, however, represent distinctly different clusters of services actually received. As was true for female youths, most members of the male youth treatment group who were enrolled in JTPA received one or both of two key services, which varied by service strategy subgroup: classroom training in occupational skills and basic education for the classroom training subgroup; on-the-job training and job search assistance for the OJT/JSA subgroup; and basic education and miscellaneous services for the other services subgroup. The treatment-control group differences in the average amount of each service received were modest.

Thus, in terms of the types of services received across service strategy subgroups and the basic patterns of services received within each subgroup, the male youths were similar to the female youths. Minor gender differences in service receipt were apparent, however, in two of the three subgroups. In the classroom training subgroup treatment-control group differences in the amount of each service received were smaller among the males than among the females. And in the other services subgroup the males were somewhat more likely than the females to receive miscellaneous services and less likely to receive basic education.

Impacts on High School Attainment. As shown in the bottom panel of Exhibit 6.10, estimated program impacts on the attainment of a training-related high school credential were positive and statistically significant both for male youths overall (with a 6.0 percentage point impact) and for those who were high school dropouts upon their application to JTPA (9.9 percentage points). The impacts were positive and statistically significant at between 9.0 and 10.1 percentage points for male youth high school dropouts in all three service strategy subgroups. A smaller proportion of the treatment group received a training-related high school credential in the OJT/JSA subgroup, however—that is, in the subgroup that placed the most emphasis on immediate employment. And the impact on the full sample in this subgroup was also smaller than on the other two subgroups.

These findings are similar to those for female youths, with two exceptions. For the OJT/JSA subgroup the finding for the female high school dropouts was small and statistically insignificant, while it was large and significant for male high school dropouts. And for the classroom training subgroup the estimated impact on high school dropouts was greater for the females than the males (16.4 percentage points versus 9.0 percentage points).

DIFFERENCES IN BASELINE CHARACTERISTICS ACROSS SERVICE STRATEGY SUBGROUPS: JTPA ASSIGNEES

Exhibit 6.11 shows selected baseline characteristics of the out-of-school male youth assignees, overall and by service strategy subgroup. As was true of the female youths, the characteristics of each service strategy subgroup of male youths were quite different, again highlighting

Exhibit 6.10 Impacts on Attainment of a Training-Related High School Diploma or GED Certificate: Male Youth JTPA Assignees Overall and High School Dropout Subgroup, by Service Strategy Subgroup

		Percentage attaining a training-related high school credential[b]		
	Sample size[a] *(1)*	*Assignees (2)*	*Control group (3)*	*Difference, in % pts. (4)*
		Classroom training subgroup		
Full sample	509	16.6%	10.3%	6.3%*
High school dropouts	302	27.3	18.3	9.0*
		OJT/JSA subgroup		
Full sample	595	5.8	2.0	3.8***
High school dropouts	236	14.9	4.9	10.1***
		Other services subgroup		
Full sample	588	18.7	11.8	6.9**
High school dropouts	417	26.1	16.9	9.1**
		All subgroups		
Full sample	1,692	13.7	7.7	6.0***
High school dropouts	955	23.9	14.0	9.9***

Sources: Unadjusted frequencies based on Background Information Form responses and First Follow-up Survey responses.

[a] Treatment and control groups combined.

[b] "Attainment of a training-related high school credential" is defined as the combination of having received some school or training serice and having attained a high school diploma or General Educational Development Certificate at some time during the 18-month follow-up period.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

the fact that it is not possible to interpret the impact findings by service strategy subgroup as indicating which service strategy would be most appropriate for the *average* male out-of-school youth.

Again, the OJT/JSA subgroup was, by almost every measure, more employable that the other two subgroups; its members were more likely to have a high school credential, to have recent work experience, and to have been married and older. This conclusion is borne out by the estimated mean earnings for each subgroup over the 12 months preceding application to JTPA (the third main panel in Exhibit 6.11): $2,742 for the OJT/JSA subgroup and only $1,779 and $1,671 for the other two subgroups, respectively. Unlike the females, however, there was not such a clear distinction between the other two subgroups of males, that is, with the classroom training subgroup tending to fall between the OJT/JSA subgroup and the other services subgroup, as it did for female youths. Among male youths the other two subgroups tended to alternate in the middle position on different measures.

Exhibit 6.11 Selected Baseline Characteristics: Male Youth JTPA Assignees, by Service Strategy Subgroup

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
Ethnicity				
White, non-Hispanic	53.7%	49.0%	69.3%	43.7%
Black, non-Hispanic	29.5	25.2	17.2	43.9
Hispanic	14.7	24.7	12.3	8.8
American Indian or Alaskan Native	1.1	0.5	0.6	2.1
Asian or Pacific Islander	1.0	0.7	0.6	1.5
Barriers to employment				
Receiving cash welfare [a]	11.0%	13.1%	10.4%	9.9%
No high school diploma or GED certificate	59.1	61.8	41.2	72.5
Worked less than 13 weeks in past 12 months	47.0	50.3	35.5	55.2
Number of barriers				
None of the above	23.9	20.3	37.3	14.3
One of the above	40.9	41.2	42.3	39.5
Two of the above	29.5	32.4	17.1	38.8
All three of the above	5.7	6.1	3.3	7.5
Work and training histories				
Ever employed	83.9%	82.8%	91.3%	78.3%
Mean individual earnings in past 12 months	$2,071	$1,779	$2,742	$1,671
Hourly earnings in most recent job				
Never employed	16.1%	17.3%	8.7%	21.7%
Less than $4	34.9	37.8	35.7	32.0
$4 or more	49.0	44.9	55.6	46.3
Employed upon application	11.4	11.5	11.7	11.0
Previously received occupational training	29.7	24.8	32.0	31.6
Public assistance status				
Receiving any public assistance [b]	29.5%	32.9%	27.6%	28.3%
Receiving AFDC	6.2	7.5	4.7	6.4
Receiving food stamps	25.0	27.3	22.0	25.7
Receiving other public assistance [c]	11.0	12.9	12.3	8.3

(Continued)

Exhibit 6.11 Selected Baseline Characteristics: Male Youth JTPA Assignees, by Service Strategy Subgroup (continued)

Characteristic	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
AFDC history				
Never AFDC case head	97.8%	97.9%	96.8%	98.7%
AFDC case head less than 2 years	1.9	2.1	3.2	0.7
AFDC case head 2 years or more	0.2	0.0	0.0	0.7
JTPA required for welfare, food stamps, or WIN program[d]	5.9%	6.5%	5.8%	5.5%
Household composition				
No spouse or own child present	85.2%	84.6%	82.2%	89.1%
Own child any age, no spouse present	4.3	6.0	2.8	3.0
Spouse present, with or without own child	10.5	9.8	15.0	7.9
Family income in past 12 months				
< $3,000	39.7%	38.7%	35.6%	44.1%
$3,000 - $6,000	26.3	27.2	26.7	25.2
$6,001 - $9,000	11.3	11.6	12.3	10.1
> $9,000	22.8	22.6	25.3	20.6
Living in public housing				
Yes	10.7%	9.8%	8.9%	12.9%
No	89.3	90.2	91.1	87.1
Age at random assignment				
16 - 19	62.1%	69.0%	51.7%	65.8%
20 - 21	37.9	31.0	48.3	34.2
Mean	*18.9*	*18.6*	*19.4*	*18.7*
Arrested since age 16[e]				
Yes	24.7%	21.4%	25.6%	26.6%
No	75.3	78.6	74.4	73.4
Sample size	*1,436*	*429*	*472*	*535*

Source: Unadjusted frequencies based on Background Information Form responses.

[a] AFDC, General Assistance, or other welfare except food stamps.

[b] "Any public assistance" includes the following sources of assistance: AFDC, food stamps, unemployment insurance, housing assistance, and other cash assistance.

[c] "Other public assistance" includes unemployment insurance, housing assistance, and other (non-AFDC) cash assistance.

[d] WIN is the federal Work Incentive program.

[e] The percentage arrested between age 16 and random assignment is based on responses to the First Follow-up Survey by treatment group members who were included in the analysis of impacts on earnings.

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As noted at the start of this chapter, by far the most striking differences between male and female youths overall were in their rates of single parenting (4 percent for males versus 36 percent for females) and their arrest rates (25 percent versus 6 percent).[23] The two genders also differed on several other measures of their employability (see columns 1 in exhibits 6.11 and 6.6)—perhaps best summarized by the difference in their average earnings in the 12 months preceding their application to JTPA: $2,071 for the males versus only $1,305 for the females. The later section detailing impacts on key subgroups will show, however, that it is the difference in arrest rates that accounts for most of the difference in program impacts between the two genders.

But the arrest rates did not vary sufficiently among service strategy subgroups to account for the differences in average impacts among these subgroups that are presented in the next subsection. Specifically, among male youths 21.4 percent of the classroom training subgroup, 25.6 percent of the OJT/JSA subgroup, and 26.6 percent of the other services subgroup reported a previous arrest. The corresponding arrest rates for female youths were 5.8 percent for classroom training, 8.5 percent for OJT/JSA, and 4.7 percent for other services (Exhibit 6.6).

IMPACTS ON EARNINGS: JTPA ASSIGNEES AND ENROLLEES IN EACH SERVICE STRATEGY SUBGROUP

Exhibit 6.12, which shows the estimated impacts on earnings for each service strategy subgroup of male youths, indicates that:[24]

- The *classroom training strategy* yielded some opportunity costs in the first quarter, possibly a small payoff in the second quarter, and then earnings close to what they would have been without JTPA in quarters 3 through 6. Over the follow-up period as a whole the estimated impact on earnings was $-259 per assignee, or -2.6 percent of the control group mean of $9,783—a small loss that was not statistically significant.

- The findings for the *OJT/JSA strategy* were very different. The estimated impacts on earnings here were negative throughout the follow-up period, for

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[23] The arrest rates shown in Exhibit 6.11 for male youths and Exhibit 6.6 for female youths are based on responses to the First Follow-up Survey, not the Background Information Form which was the data source for all the other baseline characteristics shown in those exhibits. The First Follow-up Survey did not obtain information on previous arrests among adults, however.

[24] Graphs of the monthly earnings trends on which Exhibit 6.12 is based appear as Exhibit H.11 in Appendix H. As noted earlier, the earnings and impact estimates include wages paid to JTPA participants in OJT positions. The program reimbursed employers about $600 per male youth *OJT participant* during the 18-month follow-up period. Within the OJT/JSA subgroup the reimbursement was about $190 per male youth *assignee* over the follow-up period.

Exhibit 6.12 *Impacts on Earnings: Male Youth JTPA Assignees and Enrollees, by Service Strategy Subgroup*

Period		*Control mean* *(1)*	*Impact per assignee*		*Inferred impact per enrollee, in $* *(4)*
			In $ *(2)*	*As % of (1)* *(3)*	
			Classroom training subgroup		
Quarter	1	$ 1,226	$ -300**	-24.5%	$ -441
	2	1,345	96	7.2	141
	3	1,655	-2	-0.1	-3
	4	1,773	0	0.0	-1
	5	1,889	-56	-2.9	-81
	6	1,895	4	0.2	5
All quarters		9,783	-259	-2.6	-380
			OJT/JSA subgroup		
Quarter	1	$ 1,651	$ -57	-3.4%	$ -103
	2	1,988	-219	-11.0	-398
	3	2,197	-302*	-13.8	-552
	4	2,160	-203	-9.4	-371
	5	2,316	-192	-8.3	-351
	6	2,452	-339**	-13.8	-617
All quarters		12,765	-1,313*	-10.3	-2,392
			Other services subgroup		
Quarter	1	$ 1,362	$ -285**	-20.9%	$ -432
	2	1,457	-121	-8.3	-182
	3	1,605	-218	-13.6	-330
	4	1,751	-276*	-15.7	-417
	5	1,766	-114	-6.4	-172
	6	1,899	-292**	-15.4	-442
All quarters		9,839	-1,305*	-13.3	-1,976

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 354, control group = 172; OJT/JSA subgroup: assignees = 411, control group = 204; other services subgroup: assignees = 431, control group = 176. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

a total earnings impact of $-1,313 per assignee, or -10.3 percent of the control group mean—a loss that was both large and statistically significant. Note that the control group mean was much higher for the OJT/JSA subgroup—at $12,765—than that for the other two subgroups, at around $9,800. This finding suggests that, as was true of adults and female youths, the male youths with the greatest potential earnings were recommended for the OJT/JSA strategy. In this case, however, the strategy did not serve well the male youths deemed appropriate for it.

- The impact findings for the *other services strategy* were similar to those for the OJT/JSA strategy, with negative estimated impacts throughout the follow-up period, and a negative impact for the follow-up period as a whole— $-1,305— that was almost identical to that for the OJT/JSA subgroup.

IMPACTS ON THE COMPONENTS OF EARNINGS: JTPA ASSIGNEES IN EACH SERVICE STRATEGY SUBGROUP

Exhibit 6.13 presents estimates of program impacts on the four components of the earnings impacts, displayed in columns 2 through 5.[25]

In the first panel the negative impacts in the first quarter are again consistent with the opportunity costs associated with the in-program period in the *classroom training subgroup*. There was a small gain (2.7 percent) in the percentage employed at some time during the follow-up period as a whole (column 2), but that was more than offset by the -5.4 percent drop in weeks worked per worker—to yield the -2.6 percent loss in earnings per assignee. And despite the goal of classroom occupational training to increase job-related skills, the estimated impacts on earnings per hour worked by those who worked (column 5) were small and showed no consistent trend over time.

The most striking finding in the second panel, for the *OJT/JSA subgroup*, is that the program had a negative impact on employment rates in every follow-up quarter except the first, yet the impact on employment for the follow-up period as a whole was actually positive, at 1.0 percent (although this estimate was not statistically significant). In other words, treatment group members were less likely than control group members to be employed in any given quarter, but they were slightly more likely to be employed at some time during the follow-up period. This suggests that treatment group members obtained their jobs later, held them for a shorter period, or both.

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[25] For the corresponding estimated impacts on male youths overall, see Exhibit H.10 in Appendix H. Exhibit H.9 presents impacts on the percentage employed and the number of weeks and hours worked for all male youths, including those who were never employed during the follow-up period.

Exhibit 6.13 *Percentage Impacts on Earnings and Its Components: Male Youth JTPA Assignees, by Service Strategy Subgroup*

Period	*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
			Classroom training subgroup		
Quarter 1	-24.5%	-11.6%	-10.0%	-3.9%	-1.3%
2	7.2	6.2	-3.8	3.2	1.7
3	-0.1	3.9	-2.6	-0.2	-1.1
4	0.0	0.4	-0.4	0.7	-0.7
5	-2.9	-0.3	-0.1	-2.2	-0.4
6	0.2	-2.3	-0.4	0.7	2.2
All quarters	-2.6	2.7	-5.4	0.0	0.3
			OJT/JSA subgroup		
Quarter 1	-3.4%	2.5%	1.4%	-1.8%	-5.4%
2	-11.0	-5.6	-0.6	-0.9	-4.3
3	-13.8	-6.5	-2.7	-5.0	-0.3
4	-9.4	-7.1	4.2	-6.4	0.0
5	-8.3	-3.0	-0.7	-4.5	-0.3
6	-13.8	-5.3	-1.9	-4.3	-3.0
All quarters	-10.3	1.0	-5.5	-4.0	-2.1
			Other services subgroup		
Quarter 1	-20.9%	-13.3%	-2.6%	-5.8%	-0.5%
2	-8.3	-0.3	-2.6	-3.8	-1.8
3	-13.6	-3.4	-4.9	-3.8	-2.2
4	-15.7	-6.4	-5.6	-2.4	-2.4
5	-6.4	-3.5	5.1	-0.4	-7.5
6	-15.4	-0.4	-5.7	-1.9	-8.2
All quarters	-13.3	1.9	-8.8	-2.8	-4.0

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 354, control group = 172; OJT/JSA subgroup: assignees = 411, control group = 204; other services subgroup: assignees = 431, control group = 176. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is calculated as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

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Evidence presented in Exhibit H.23 of Appendix H on the amount of time between random assignment and the first job obtained by youths in the sample suggests that the OJT/JSA service strategy may have slightly delayed employment for male youths (although the impact estimate was not statistically significant). Our final report will present a further analysis of the timing and duration of job spells for members of each service strategy subgroup.

A final point to consider is that most of the program induced earnings loss experienced by male youths in the OJT/JSA subgroup was due to the fact that treatment group members worked fewer weeks than control group members did during the 18-month follow-up period and treatment group members worked fewer hours per week employed.

Returning to Exhibit 6.13, we find a similar pattern for the *other services subgroup*, although the quarter-by-quarter impacts on the percentage employed tend to more closely parallel those for the classroom training subgroup. There was a sharp initial decline in employment in the first quarter (consistent with a short investment of time in the program) and continued negative employment effects through the remaining five quarters. The estimated effects on weeks and hours worked for those employed were negative throughout the follow-up period, as were the estimated effects on earnings per hour worked, especially in the last two quarters. And although the estimated impacts on quarterly employment rates were negative, the overall impact on the employment rate for the follow-up period as a whole was positive (though not statistically significant). In all three service strategy subgroups the estimated negative impact on weeks worked per worker was the most salient component of the overall impact on earnings.

Impacts on Earnings: Female and Male Youths in Selected Key Subgroups

This section presents estimates of JTPA impacts on the 18-month earnings of selected key subgroups of youths. The majority of the key subgroups of youths are defined in the same way as were the key subgroups of adult women and men (exhibits 4.15 and 5.14); but we have included two additional sets of subgroups to examine potential differences in program impacts with respect to characteristics that we expected to be more relevant for youths than for adults, namely, previous occupational training and reported arrests.[26]

The analysis attempts to identify subgroups for which the JTPA Title II-A program was effective in the 16 study sites and subgroups for which the program was not effective.

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[26] We selected the key subgroups examined in chapters 4,5, and 6 based on their relevance to policy discussions, before we calculated the estimates. In other words, we did not select them on the basis of the size or significance of the program impact estimates.

Knowledge of the former groups can facilitate future research on the factors that lead to program success, while knowledge of the later can help target efforts to improve the program. But the analysis by itself cannot determine why the program was working for some groups and not for others. Nor does it lead to simple prescriptions about how to improve the program. In short, it can only measure the effects of the program, the way it actually operated, on the people it actually served.

Exhibits 6.14 and 6.15 present the findings for female and male youths in the same way that corresponding findings were presented in chapters 4 and 5. Each panel in the exhibits defines a set of subgroups in terms of a particular set of baseline characteristics. For example, the first panel defines subgroups according to the ethnicity of sample members; the second, according to specific employment barriers faced by sample members; and the third, according to their work histories.

The first column in the exhibits presents the sample size for each subgroup, including both treatment group and control group members. The second column presents the mean earnings of control group members over the 18-month follow-up period. Column 3 displays estimates of the average program impacts on total 18-month earnings for each subgroup. Asterisks beside the impact estimates denote that the estimates are statistically significantly different from zero, whereas asterisks in the final row in each panel indicate that impact estimates in the panel are statistically significantly different from one another.

The fourth column in the exhibits displays the column 3 estimates adjusted for differences in the distributions of the subgroups across the 16 study sites. These estimates control statistically for the extent to which some subgroups were more heavily concentrated in sites with more positive or negative impacts than the other subgroups in the panel. Likewise the fifth column displays the column 3 estimates adjusted both for differences in subgroup distributions across sites and for differences in subgroup distributions across service strategies.[27]

To interpret the findings in the exhibit one should proceed as follows. First, to assess the likely impact for any given subgroup, examine the size and significance of the impact estimates in column 3. Estimates that are statistically significant are those that are most likely to represent true impacts for a subgroup, as opposed to chance results due to random sampling error. One should place the most confidence in these significant estimates. The significance of the *variation* in impacts among subgroups is measured by an F-test, with results reported in the last row of each panel. Those panels that contain subgroup impact

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[27] The estimates in columns 4 and 5 of exhibits 6.14 and 6.15 adjust the distribution of each subgroup to equal the distribution of female or male youths overall across sites (column 4) or across sites and service strategies (column 5). For a full description of the methodology for deriving these estimates, see Appendix D.

Exhibit 6.14 Impacts on Total 18-Month Earnings: Female Youth JTPA Assignees, by Selected Key Subgroup

				Impact, in $, adjusted for sample distribution across:	
Key subgroup, defined by:	*Sample size*[a] *(1)*	*Control mean (2)*	*Impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
Ethnicity					
White, non-Hispanic	1,148	$ 7,076	$ -122	$ -348	$ -231
Black, non-Hispanic	749	5,601	-135	455	518
Hispanic	366	5,019	-554	-338	-147
F-test, difference among subgroups			n.s.	n.s.	n.s.
Barriers to employment (in italic)					
Receiving cash welfare [b]	701	4,397	-391	-515	-393
No cash welfare[b]	1,412	7,174	-154	-310	-287
F-test, difference between subgroups			n.s.	n.s.	n.s.
No high school diploma or GED certificate	1,047	4,192	23	42	223
High school diploma or GED certificate	1,146	8,055	-437	-346	-232
F-test, difference between subgroups			n.s.	n.s.	n.s.
Worked less than 13 weeks in past 12 months	1,235	4,425	-31	-34	109
Worked 13 weeks or more in past 12 months	829	8,886	-255	-331	-198
F-test, difference between subgroups			n.s.	n.s.	n.s.
Number of barriers					
None of the above	545	9,964	-260	-303	-278
One of the above	790	6,552	-236	-265	-149
Two of the above	675	4,486	-451	-496	-353
All three of the above	281	2,189	659	716	934
F-test, difference among subgroups			n.s.	n.s.	n.s.
Work history					
Never employed	514	3,201	-232	1	192
Earned < $4 hourly in last job	1,073	6,447	-83	-108	-36
Earned $4 or > hourly in last job	713	8,030	-277	-235	-111
F-test, difference among subgroups			n.s.	n.s.	n.s.
Employed upon application	340	9,794	-218	-351	-178
Not employed upon application	1,950	5,595	-172	-93	5
F-test, difference between subgroups			n.s.	n.s.	n.s.
Previous occupational training					
Yes	582	7,210	-441	-301	-274
No	1,670	5,838	-83	-169	-15
F-test, difference between subgroups			n.s.	n.s.	n.s.

(Continued)

Exhibit 6.14 Impacts on Total 18-Month Earnings: Female Youth JTPA Assignees, by Selected Key Subgroup (continued)

				Impact, in $, adjusted for sample distribution across:	
Key subgroup, defined by:	*Sample size* [a] *(1)*	*Control mean (2)*	*Unadjusted impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
AFDC history					
Never AFDC case head	1,634	$ 6,656	$ -232	$ -248	$ -141
AFDC case head less than 2 years	487	5,280	269	349	503
AFDC case head 2 years or more	162	4,125	-1,089	-1,028	-984
F-test, difference among subgroups			n.s.	n.s.	n.s.
JTPA required for welfare, food stamps, or WIN program [c]					
Yes	163	2,994	342	193	234
No	2,029	6,518	-204	-249	-143
F-test, difference between subgroups			n.s.	n.s.	n.s.
Household composition					
No spouse or own child present	1,111	6,979	-327	-308	-225
Own child any age, no spouse present	789	5,796	-380	-272	-149
Spouse present, with or without own child	252	4,819	1,007	917	1,029
F-test, difference among subgroups			n.s.	n.s.	n.s.
Family income in past 12 months					
$6,000 or less	1,524	6,011	-487	-536	-437
More than $6,000	557	7,160	511	419	519
F-test, difference between subgroups			n.s.	n.s.	n.s.
Living in public housing					
Yes	307	4,077	246	238	292
No	1,939	6,602	-288	-340	-249
F-test, difference between subgroups			n.s.	n.s.	n.s.
Age at random assignment					
16 - 19	1,400	5,732	-124	-101	19
20 - 21	900	6,999	-279	-219	-114
F-test, difference between subgroups			n.s.	n.s.	n.s.
Arrested since age 16					
Yes	125	5,827	705	689	741
No	2,122	6,251	-200	-154	-19
F-test, difference between subgroups			n.s.	n.s.	n.s.
Recommended for JSA only					
Yes	49	7,366	1,321	1,071	n/a
No	2,251	6,174	-209	-206	n/a
F-test, difference between subgroups			n.s.	n.s.	n.s.
Full sample	2,300	6,225	-182	-182	-182

Source: Estimates based on First Follow-up Survey responses.

Notes: Estimates are regression-adjusted to the control for differences in baseline characteristics between the treatment group and control. group; see Appendix D. Control means are not regression-adjusted. Sample sizes for mutually exclusive subgroups within a panel do not necessarily sum to the sample size for the target group as a whole, because persons in omitted subgroups or with missing data on the variable used to define the subgroup are excluded.

a. Treatment and control groups combined.

b. AFDC, General Assistance, or other welfare except food stamps.

c. WIN is the federal Work Incentive program.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the F-test for the difference in impacts between or among the subgroups in the category is not statistically significant.

estimates that are statistically significantly different from one another provide the strongest evidence that the true impacts for these subgroups were actually different. If the subgroup impact estimates within a panel are not statistically significantly different from one another, the fact that their point estimates differ does not provide strong evidence that the true impacts for the subgroups were actually different.[28]

Having examined the column 3 estimates for a set of subgroups in a panel, one should next read across the rows to their counterparts in the last two columns. To the extent that the variation in impact estimates across subgroups changes as one moves from the column 3 estimates to the estimates in columns 4 and 5, then effects related to sites or sites and service strategies explain part of the subgroup variation. In other words, the fact that one or more of the subgroups were concentrated in different sites or recommended for different service strategies explains part of the variation in estimated impacts. But if the variation in impact estimates shown in column 3 remains unchanged as one moves to the corresponding estimates in columns 4 and 5, differences in the distributions of the subgroups across sites or across sites and service strategies do not explain the variation in impacts among subgroups.

The following discussion of the findings in exhibits 6.14 and 6.15 focuses primarily on subgroup impact estimates that are statistically significantly different from zero and statistically significantly different from one another, because one can place the most confidence in these estimates. We begin each subsection, however, by discussing one set of subgroup estimates that are *not* statistically significantly different from one another—those for ethnic groups—because this lack of a difference is an important finding to note.

Female Youths in Key Subgroups

The top panel in Exhibit 6.14 shows that program impact estimates for the three main ethnic groups of female youths—whites, blacks, and Hispanics—were neither statistically significant nor significantly different from one another. Thus, there no evidence that the Title II-A programs studied produced different impacts on the 18-month earnings of these subgroups.

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[28] All the subgroup impact estimates in exhibits 6.14 and 6.15 are based on ordinary least squares regressions on a pooled sample of all female youths and male youths, respectively, with the treatment indicator interacted with the defining characteristic of the subgroup and (as appropriate) site or site and service strategy. This approach allowed direct calculation of the F-test for differences in impacts among subgroups in each panel of the exhibits. Subgroup impacts were also estimated on samples containing only the subgroup of interest (not shown); in general, these "split file" estimates differed little from those based on the pooled regressions.

Indeed, although most of the estimates in Exhibit 6.14 were slightly to moderately negative (a few were positive), none was statistically significant. Moreover, within panels the impact estimates for each set of subgroups were not significantly different from one another. The overall pattern of estimates in the exhibit therefore suggests that the JTPA program had little or no impact on any of the subgroups of female youths identified for this analysis.

MALE YOUTHS IN KEY SUBGROUPS

The top panel of the next exhibit, 6.15, shows large negative estimated program impacts on the earnings of white and Hispanic male youths, and the estimate for white male youths was statistically significant. In contrast, the estimated impact on black male youths is close to zero. Despite these apparent differences among the main ethnic groups, however, the estimates were not statistically significantly different from one another. These findings mean we have confidence that the estimate for white male youths reflects a real program impact on that subgroup, but we cannot be sure whether the impacts for blacks and Hispanics were different from that for whites or were instead due to random sampling error. The column 3 estimates for blacks and especially Hispanics did change, when adjusted for the distributions of the two subgroups across sites (column 4) or sites and service strategies (column 5), but in neither case were the differences among ethnic groups statistically significant. There is no evidence, therefore that, JTPA produced systematically different impacts for the three main ethic groups of male youths.

The remaining panels in Exhibit 6.15 present a pattern that is quite different from those for subgroups of female youths in the previous exhibit. Many subgroups of male youths had large, negative—and statistically significant—impact estimates; only 4 of 35 subgroup estimates in column 3 were positive, and none of these are statistically significant.

But only two sets of male youth subgroups had findings within the set that were significantly different from each other: the subgroups of male youths recommended for job search assistance only versus those not recommended only for this service (the last panel); and the subgroups of male youths who had been arrested between their sixteenth birthday and the time they applied to JTPA versus those who had not been arrested.

Because only 77 treatment and control group members (forming less that 5 percent of the male youth sample overall) were recommended for job search assistance (JSA) only, it is difficult to know what to make of the estimated $5,402 program-induced earnings loss for this subgroup. Furthermore, because of its small size, removing the subgroup from the male youth sample did not seriously alter the impact estimate for the larger group,

Exhibit 6.15 Impacts on Total 18-Month Earnings: Male Youth JTPA Assignees, by Selected Key Subgroup

				Impact, in $, adjusted for sample distribution across:	
Key subgroup, defined by:	*Sample size*[a] *(1)*	*Control mean (2)*	*Impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
Ethnicity					
White, non-Hispanic	946	$ 12,550	$ -1,333**	$ -1,680**	$ -1,995***
Black, non-Hispanic	522	8,164	75	-506	-414
Hispanic	248	10,126	-1,238	-21	213
F-test, difference among subgroups			n.s.	n.s.	n.s.
Barriers to employment (in italic)					
Receiving cash welfare [b]	185	8,815	-56	-357	-578
No cash welfare[b]	1,374	11,292	-1,020**	-1,060*	-1,153*
F-test, difference between subgroups			n.s.	n.s.	n.s.
No high school diploma or GED certificate	947	10,087	-1,144*	-1,471**	-1,454**
High school diploma or GED certificate	730	11,612	-420	-279	-506
F-test, difference between subgroups			n.s.	n.s.	n.s.
Worked less than 13 weeks in past 12 months	754	8,616	-1,286**	-1,332*	-1,460*
Worked 13 weeks or more in past 12 months	842	12,808	-832	-829	-1,011
F-test, difference between subgroups			n.s.	n.s.	n.s.
Number of barriers					
None of the above	475	13,352	-459	-324	-506
One of the above	733	10,810	-695	-765	-839
Two of the above	455	8,520	-1,242	-1,481	-1,531*
All three of the above	81	7,642	-1,278	-1,600	-1,743
F-test, difference among subgroups			n.s.	n.s.	n.s.
Work history					
Never employed	269	7,858	-1,067	-1,233	-1,249
Earned < $4 hourly in last job	617	9,687	-745	-969	-1,095
Earned $4 or > hourly in last job	862	12,435	-808	-842	-943
F-test, difference among subgroups			n.s.	n.s.	n.s.
Employed upon application	213	11,588	526	266	163
Not employed upon application	1,521	10,722	-1,073**	-1,213**	-1,329**
F-test, difference between subgroups			n.s.	n.s.	n.s.
Previous occupational training					
Yes	527	11,903	-575	-585	-769
No	1,183	10,285	-904*	-1,049	-1,118*
F-test, difference between subgroups			n.s.	n.s.	n.s.

(Continued)

Exhibit 6.15 Impacts on Total 18-Month Earnings: Male Youth JTPA Assignees, by Selected Key Subgroup (continued)

				Impact, in $, adjusted for sample distribution across:	
Key subgroup, defined by:	*Sample size* [a] *(1)*	*Control mean (2)*	*Impact, in $ (3)*	*Sites (4)*	*Sites and service strategies (5)*
JTPA required for welfare, food stamps, or WIN program [c]					
Yes	91	$ 7,178	$ 462	$ 552	$ 542
No	1,552	10,883	-814*	-947	-1,052*
F-test, difference between subgroups			n.s.	n.s.	n.s.
Household composition					
No spouse or own child present	1,411	10,679	-1,176**	-1,238**	-1,375**
Own child any age, no spouse, present	70	10,670	-2,438	-2,641	-2,762
Spouse present, with or without own child	181	12,927	1,522	1,660	1,749
F-test, difference among subgroups			n.s.	*	*
Family income in past 12 months					
$6,000 or less	1,038	10,495	-868	-978	-1,076
More than $6,000	544	11,500	-833	-883	-948
F-test, difference between subgroups			n.s.	n.s.	n.s.
Living in public housing					
Yes	188	9,564	-956	-1,185	-1,298
No	1,506	10,905	-722	-759	-852
F-test, difference between subgroups			n.s.	n.s.	n.s.
Age at random assignment					
16 - 19	1,053	10,464	-1,314**	-1,795***	-1,881***
20 - 21	695	11,269	-179	-177	-288
F-test, difference between subgroups			n.s.	*	*
Arrested since age 16					
Yes	401	11,237	-3,038***	-3,150***	-3,254***
No	1,313	10,696	-224	-219	-342
F-test, difference between subgroups			***	***	***
Recommended for JSA only					
Yes	77	15,534	-5,402**	-6,297**	n/a
No	1,671	10,649	-687	-801	n/a
F-test, difference between subgroups			**	**	
Full sample	1,748	10,736	-854**	-854**	-854**

Source: Estimates based on First Follow-up Survey responses.

Notes: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Control means are not regression-adjusted. Sample sizes for mutually exclusive subgroups within a panel do not necessarily sum to the sample size for the target group as a whole, because persons in omitted subgroups or with missing data on the variable used to define the subgroup are excluded.

a. Treatment and control groups combined.

b. AFDC, General Assistance, or other welfare except food stamps.

c. WIN is the federal Work Incentive program.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the F-test for the difference in impacts between or among the subgroups in the category is not statistically significant.

reducing the $-854 average earnings loss for all male youths to a still-large (but statistically insignificant) $-687 earnings loss for those male youths who were not recommended for JSA only (results shown in the exhibit).

The impact estimate for the subgroup of male youths with a prior arrest (401 treatment and control group members) accounts, however, for a major portion of the estimated impact on the male youth sample. Removing this subgroup from the sample reduced the significant $-854 average earnings loss for all male youths to a statistically insignificant $-224 earnings loss for male youths without a previous arrest. The estimated impact for previous arrestees was a highly significant $-3,038 earnings loss, as shown in column 3—a result that was significantly different, at the .01 level, from the estimate for the nonarrested subgroup. In short, the *25 percent of the male youths who had been arrested before their JTPA application accounted for about 82 percent of the total program-induced earnings loss estimated for male youths overall.*[29]

Our forthcoming final report will explore further the potential sources and implications of this striking result. The limited analyses we have conducted to date (not shown here) suggest the following:

- Male youths with previous arrest experienced large, negative program impacts on their earnings *in all six follow-up quarters*. These estimates were statistically significant in five of the six follow-up quarters. Since the median duration of enrollments for this subgroup was relatively short (3.3 months), little of this earnings loss is likely to have been a result of time spent in training.

- The impact estimates were also large and negative for *all three service strategy subgroups* of previous arrestees, at $-3,420 for the classroom training subgroup, $-5,746 for the OJT/JSA subgroup, and $-2,200 for the other services subgroup. The estimates for the classroom training and OJT/JSA subgroups were statistically significant, and that for the other services subgroup was significant at a near-conventional level (.15).

- The impact estimates for this subgroup were also negative in *13 of the 15 study sites,*[30] although because of the small sample sizes involved these estimates were not statistically significant. As a result, the overall earnings loss for the previous arrestees is unlikely to be merely the results of the idiosyncrasies of only a few sites.

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[29] The 82 percent figure represents the total program-induced earnings loss for male youths with a previous arrest expressed as a percentage of the total earnings loss for all male youths. The total earnings loss for each of these groups was computed as the product of the average earnings loss per treatment group member times the number of treatment group members within the group.

[30] The Oakland site excluded youths from the experiment; thus, there are 15 instead of 16 study sites for youths in the National JTPA Study.

- The *types of jobs* the previous arrestees reported during the 18-month period were mainly low-wage positions in service industries such as fast foods and maintenance and repair. These jobs are plausible types of jobs for this subgroup, which suggests that the previous arrestees' response to the First Follow-up Survey (the data source for the impact analysis in this report) were not exaggerated.

- Most of the observed earnings loss for the previous arrestees remained when those with the *highest earnings* (the "outliers") were removed from the analysis. The impact findings for this subgroup therefore represent more than extreme results for a few sample members.

But despite the apparent consistency of all these estimates for previous arrestees within the male youth sample, our preliminary analyses also uncovered an important reason for exercising caution in interpreting these estimates. In particular, we have determined that these estimates, which are based on sample members' response to the First Follow-up Survey, differ systematically and significantly from impact based on an alternative data source: earnings from state unemployment insurance (UI) agencies.

Appendix E presents a detailed comparison of estimates based on the two data sources—including estimates of earnings, employment, and program impacts on both earnings and employment—for all four target groups. Although the survey-based estimates differ somewhat from the UI-based estimates for all four target groups, the two sets of estimates differ in a similar way for both the treatment group and the control group—except in the case of the 401 male youths with a previous arrest.

In other words, although the UI-based estimates of sample members' earnings differ from the survey-based estimates in absolute magnitude, the UI-based and survey-based estimates of program *impacts* did *not* differ greatly for adult women, adult men, or female youths overall, or for male youths who reported never having been arrested. But the UI-based estimated impact on the earnings of male youths with a previous arrest *is* statistically significantly different from the survey-based estimate. Specifically, the UI-based impact estimate is a statistically insignificant $34, whereas the survey-based estimate, as we have seen, is a statistically significant $-3,038.

At present we do not fully understand the reasons for these ambiguous findings; our forthcoming final report will include further analysis that should supply us with a better understanding. In the meantime, then, the survey-based finding of a large, negative, and statistically significant program impact on the earnings of the previous arrestees among male youths must be interpreted with caution. And even more important, because most of the negative impact estimated for male youths *overall* is due to the extremely large negative impact on the subgroup of male youths with a previous arrest, our findings for male youths overall must also be interpreted with caution.

At the same time, however, neither the survey-based nor the UI-based estimates indicate a program-induced earnings gain for male youths. Thus, at the very least we can conclude that, as was the case for female youths, the JTPA Title II-A program did not have a positive impact on the earnings or employment of male youths.

A Summary of Impacts, in the Context of Previous Research

All told, the estimated impacts of JTPA Title II-A on out-of-school youths present a picture of a program that was not working to increase the earnings and employment of those out-of-school youths at the 16 sites studied but that was achieving success in increasing the likelihood that those youths without a high school credential would obtain one over the 18 months of follow-up that we have studied so far. This section summarizes the findings for youths in more detail and then compares them with results from the two previous experimental studies of employment and training programs for out-of-school youths. The findings from the three studies are not inconsistent with one another, and together they form the most reliable body of knowledge that is available on the effectiveness of employment and training programs for economically disadvantaged out-of-school youths.

FEMALE YOUTHS

As summarized in the first two rows of Exhibit 6.16, JTPA had a negligible impact on the *earnings* of the 2,323 female youths in the study sample: It reduced earnings $-182, or -2.9 percent over the 18-month follow-up period. The loss was not statistically significant, however.

Although the estimated impacts on earnings differed across the three service strategy subgroups of female youths (column 2 through 4), again none was statistically significant. Specifically, the estimated impacts were a loss of $-542 (-9.1 percent) for the *classroom training subgroup*, a gain of $410 (5.4 percent) for *the OJT/JSA subgroup*, and a loss of $-158 (-2.8 percent) for the *other services subgroup*.

The next five rows of Exhibit 6.16 show that the estimated impacts on *employment* were also for the most part negligible for female youths. The estimated impacts on

Exhibit 6.16 Summary of Estimated JTPA Impacts on Earnings and Employment over the Full Follow-up Period: Out-of-School Youth JTPA Assignees in the 18-Month Study Sample, by Gender and Service Strategy Subgroup

Impact per assignee on:	*All subgroups (1)*	*Classroom training subgroup (2)*	*OJT/ JSA subgroup (3)*	*Other services subgroup (4)*
		Female youths		
Earnings				
In $	$ -182	$ -542	$ 410	$ -158
As % of control mean	-2.9%	-9.1%	5.4%	-2.8%
Percentage employed [a]	2.8%	3.5%	3.6%	2.6%
Weeks worked				
In weeks	-1.6	-2.4	-1.4	-0.4
As % of control mean	-4.6%	-7.2%	-3.4%	-1.3%
Hours worked				
In hours	-61	-119*	-13	-4
As % of control mean	-4.7%	-9.7%	-0.8%	-0.4%
Sample size [b]	*2,300*	*1,045*	*545*	*710*
		Male youths		
Earnings				
In $	$ -854**	$ -259	$ -1,313*	$ -1,305*
As % of control mean	-7.9%	-2.6%	-10.3%	-13.3%
Percentage employed [a]	1.5%	2.4%	0.9%	1.6%
Weeks worked				
In weeks	-2.3*	-1.3	-2.3	-3.0
As % of control mean	-4.9%	-2.9%	-4.5%	-7.1%
Hours worked				
In hours	-129**	-50	-182	-169
As % of control mean	-6.8%	-2.9%	-8.3%	-9.6%
Sample size [b]	*1,748*	*526*	*615*	*607*

Source: Estimates based on First Follow-up Survey responses.

Note: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

[a] At any time during the 18-month follow-up period. The impact is measured in percentage points.

[b] Treatment and control groups combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). For the impacts on earnings, weeks worked, and hours worked, the significance level of each estimate expressed "as % of control mean" is the same as that of the corresponding estimate expressed in dollars, in weeks, or in hours.

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employment rates were small but positive for female youths overall, at 2.8 percentage points, and for all three other services subgroup to 3.6 percentage points for the OJT/JSA subgroup. The impacts on the number of *weeks* and *hours worked*, averaged over all sample members, were, however, negative for the target group overall and for all three service strategy subgroups. But none of these impacts on employment was statistically significant, except for the -119 (-9.7 percent) loss in hours worked per week worked for female youths in the classroom training subgroup. Together these findings suggest that JTPA had no significant impact on employment for female youths—not in the ability to find a job, how quickly jobs were found or how long they were held, or the extent of full-time jobs—but the program did have an effect on the extent of full-time work for those female youths who were recommended for classroom training.[31]

Despite the negligible impacts on earnings and employment for female youths, access to JTPA had a highly significant impact on *attainment of a training-related high school diploma or GED certificate*, for female youth overall and for those in the classroom training and other services subgroups—the two subgroups in which basic education was a key service received. Among those female youth who were high school dropouts upon application to JTPA, the estimated impacts on this form of educational attainment were 11.9 percentage points for female youths overall and 16.4 and 10.7 percentage points for the classroom training and other services subgroups, respectively. Recall that the JTPA performance standards for youths are in essence twofold, emphasizing both employment outcomes and education and training outcomes. Hence, the estimated impact on high school attainment relates to a central objective of the program.

For the *key subgroups* of female youths, selected for their relevance to policy and program planning debates, none of the estimated impacts on 18-month earnings was statistically significant; nor were any of the subgroups within a related set of subgroups significantly different from one another.

MALE YOUTHS

As shown in the bottom panel of Exhibit 6.16, JTPA had a negative impact on the *earnings* of the 1,773 male youths in the study sample: It reduced their earnings by $-854, or -7.9 percent over the 18-month follow-up period. That loss, which represents an average over all male youth treatment group members, was statistically significant.

But most of the negative impact on earnings (82 percent) was concentrated in the 25 percent of male youths who reported having been arrested between age 16 and their random assignment. Members of this subgroup experienced a highly significant loss of $-3,038, on average, or 27.0 percent of what they would have earned without access to JTPA. In contrast, the male youths who reported no previous arrests (75 percent of the total) experienced an estimated $-224, or 2.1 percent, earnings loss. Hence, the impact on earnings for *most* male youths was similar to that for female youths: It was negligible.

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[31] Exhibit H.4 in Appendix H, which displays estimated impacts on the components of the earnings impact for female youths overall, suggests that the impact on earnings represents a combination of a 3.6 percent impact on the employment rate and a 1.9 percent impact on earnings per hour worked, which together are offset by a -7.9 impact on weeks worked per worker.

The estimated average impacts on earnings were negative across all three service strategy subgroups of male youths, but they were most negative for the OJT/JSA and other services strategies. Male youths recommended for the *OJT/JSA strategy* had an earnings loss of $-1,313 (-10.3 percent), while those recommended for the *other services* strategy had an earnings loss of $-1,305 (-13.3 percent). Both of these estimated impacts were statistically significant. Male youths recommended for the *classroom training strategy*, on the other hand, had a statistically insignificant loss of only $-259 (-2.6 percent). In all three service strategy subgroups, however, those male youths with a previous arrest experienced more extreme negative impacts than did those without a previous arrest.[32]

The last five rows of Exhibit 6.16 present mixed evidence of JTPA impacts on *employment* for male youths. The estimated impacts on *employment rates* were small, positive, and not statistically significant for male youths overall and for all three service strategy subgroups. The impacts on the number of *weeks and hours worked*, averaged over all sample members, were, however, negative and statistically significant.

Male youths overall appear to have experienced a loss of -2.3 weeks of work (-4.9 percent) and a loss of -129 hours of work (-6.8 percent). The estimated impact on the average number of weeks and hours worked were also negative for all three service strategy subgroups, but they were not statistically significant. Together these findings, along with those on program impacts on the components of earnings in Exhibit H.10 (Appendix H), suggest that the earnings loss for male youths was primarily a result of negative impacts on the ability to find a job quickly, how long jobs were held, and the extent of full-time employment, not the result of a negative impact on the ability to find a job per se.

Despite the negligible impacts on earnings for most male youths and the extremely negative impacts on earnings for those with a previous arrest, JTPA again had a highly significant impact on *attainment of a training-related high school credential* for male youths, as it did for female youths. The impacts were positive and statistically significant for all three service strategy subgroups. Specifically, for male youths who were high school dropouts the likelihood of attaining a training-related high school credential was increased by 9.9 percentage points for male youths overall and by between 9.0 and 10.1 percentage points for each of the three service strategy subgroups.

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[32] As noted earlier in the chapter, the findings for male youths with a previous arrest that are based on First Follow-up Survey responses are inconsistent with findings for a subsample of male youths estimated based on earnings data from state unemployment insurance agencies (see Appendix E). Our forthcoming final report will further investigate this discrepancy, but note that neither data source yields findings suggesting positive impacts on earnings for male youths with or without a previous arrest. Also note that the impact findings from the two data sources were not inconsistent for adult women, adult men, female youths, and male youths who did not have a previous arrest.

Finally, we have already mentioned the most prominent finding from our analysis of *key subgroups* of male youths, namely, that most of the negative average impact estimated for male youths overall was concentrated among those who reported having been arrested since turning the age of 16.

PREVIOUS EXPERIMENTAL FINDINGS ON PROGRAMS FOR OUT-OF-SCHOOL YOUTHS

To provide a broader context in which to assess the impacts reported above, Exhibit 6.17 displays our 18-month findings for JTPA along with findings from the two other major randomized experiments that have been conducted to date on employment and training programs for out-of-school youths: the JOBSTART demonstration (Cave and Doolittle, 1991), and the youth component of the National Supported Work Demonstration (Maynard, 1980).

Columns 1 and 2 show that although the three studies have much in common, the samples of out-of-school youths studied differed somewhat across the three studies, and services provided to those youths differed even more. JOBSTART services, for example, were similar to those in JTPA but were more intensive and did not include on-the-job training, whereas Supported Work offered structured, paid work experience exclusively, a service that was rarely provided to the out-of-school youths in JTPA. The comparison below is therefore only in the most general of terms.

Columns 3 through 5 present the impact estimates. As in exhibits 4.17 and 5.16 in chapters 4 and 5, the estimates are expressed as *average quarterly impacts* during each of the three *years* following sample members' random assignment and the estimates from the previous studies have been converted to July 1989 dollars.

The JOBSTART demonstration, conducted at 13 sites in the mid-1980s, tested an intensive program of basic education, classroom occupational skills training, support services (mainly child care and transportation), and job search assistance for economically disadvantaged high school dropouts, ages 17 to 21, who read below the eighth grade level. This study sample was about evenly divided by gender; and 5 percent of the females in the treatment group and 29 percent of the males had a previous arrest.

Exhibit 6.17 *Estimated Impacts on the Average Quarterly Earnings of Out-of-School Youths: The JTPA 18-Month Impact Analysis and Previous Experimental Studies*

Program (Year evaluation began)	Treatment group (1)	Program services (2)	Quarterly $ impact per treatment group member (% impact in parentheses) Year 1 (3)	Year 2[a] (4)	Year 3 (5)
National JTPA Study (1987)	Low-income youths, ages 16-21, 44% male; participation voluntary Females: 49% with no HS diploma/GED certif., 27% receiving AFDC, 5% arrested since age 16 Males: 59% with no HS diploma/GED certif., 6% receiving AFDC, 20 % arrested since age 16	Classroom training strategy: occupational skills training, basic education[b]	%-131** (-14%)	Females $-9 (-1%)	--
			$-52 (-3%)	Males $-26 (-1%)	–
		OJT/JSA strategy: on-the-job training, job search assistance	$113 (10%)	Females $-22 -2%	–
			$ -119 (-10%)	Males $-226* (-11%)	–
		Other services strategy:[b] basic education,[c] miscellaneous	$-43 (-5%)	Females $7 (1%)	–
			$-225* (-15%)	Males $-203 (-11%)	--
		All three strategies	$-44 (-5%)	Females $-3 (0%)	–
			$-140 (-5%)	Males $-147* (-7%)	–
JOBSTART Demonstration[d]* (1985)	Low-income HS dropouts reading below the 8th grade level, ages 17-21, 47% male; participation voluntary Females: 33% receiving AFDC, 5% ever arrested Males: 6% receiving AFDC, 29% ever arrested	Basic education,[b] occupational skills training, support services, job search assistance	$66[e] -15%	Females $58 (9%)	--
			$-284[e] (-28%)	Males $-177** -12%	–
National Supported Work Demonstration[f]** (1976)	Low-income HS dropouts, ages 17-20, 88% male; participation voluntary Female and male treatment group: 13% receiving AFDC Female and male Supported Work enrollees: 57% ever arrested	Structured, paid work experience for up to 12 months	– [g]	Females and Males $22[g] (5%)	$-42[g] (-7%)

(continued)

The JOBSTART findings for the first two years after random assignment basically parallel our JTPA findings. The estimated impacts on the earnings of female youths were negligible and not statistically significant in years 1 and 2 ($66 and $58), whereas the impacts on the earnings of male youths were negative in both years ($-284 and $-177) and statistically significant in the second.

These estimates are of particular interest not only for their similarities to the estimates for JTPA shown in the top panel, but also because of certain similarities between JOBSTART and JTPA participants and programs. About half of the out-of-school youths in the JTPA 18-month study sample were high school dropouts, and 5 percent of the females and 25 percent of the males in the treatment group were previous arrestees. Moreover, the JOBSTART demonstration was conducted within the JTPA system, and its basic services—though more intensive than the average for JTPA—were similar in many ways to those in its program for youths.

The National Supported Work Demonstration, a component of which was conducted with out-of-school youths in five sites during the late 1970s, studied a lengthy, intensive, and highly structured program of paid work experience. The study treatment group comprised economically disadvantaged high school dropouts, ages 17 to 20, who were mostly male (88 percent) and a large proportion of whom (57 percent) had a previous arrest.

The exhibit does not show impact estimates for the first year after random assignment for Supported Work because most treatment group members were in the program and receiving heavily subsidized earnings during that period. The estimates for the second and third years after random assignment, when most treatment group members were out of the program, were negligible ($22 and $-42) and statistically insignificant.

Sources for Exhibit 6.17:

+ Cave and Doolittle (1991, tables 3.9, 5.5, 5.6, and E.2, pp. 87-88, 145, 147, and 247).

++ Maynard (1980, tables II.3 and A.1, pp. 24 and 153); Kemper and Long (1981, table VIII. 3, p. 219); and Kemper, Long, and Thornton (1981, table VI. 1, p. 148).

Notes: The reported estimates from previous studies have been converted to July 1989 dollars. The "% impact" is the "$ impact" expressed as a percentage of the control group mean.

a. The estimates from the National JTPA study (first panel) in this column are for the first two quarters only.

b. For JTPA "basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL). For JOBSTART "basic education" includes GED preparation only.

c. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

d. Included demonstrations in 13 sites, with a net cost (in nominal dollars) per treatment group member of $4,611.

e. Tests of the statistical significance of this estimate were not performed.

f. Included demonstrations in five sites, with a net cost per treatment group member (in 1976 dollars) of $5,982, or $7,587 including revenues generated by the demonstrations.

g. Impact estimates are not presented for year 1 after random assignment because most treatment group members were in the program and thereby receiving heavily subsidized earnings during that year.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Thus, despite important differences in the program participants and services studied in the three analyses, their comparison is nonetheless instructive. The three programs studied represent a broad range of efforts to improve the earnings and employment for out-of-school youths, and all three studies used a rigorous evaluation methodology. Taken together, these studies lead to the conclusion that our nation has not yet found a solution to the employment and training problems of its economically disadvantaged out-of-school youths.

7

A Comparison of JTPA Impacts across Target Groups and Study Sites

THE three previous chapters examined the estimated effects of JTPA on four different target groups—adult women, adult men, female youths, and male youths—taking each target group in turn. This chapter reviews the main findings of those chapters side by side, looking for similarities and differences. Of particular interest is the sharp contrast between the estimated impacts on adults and youths. The chapter also presents separate findings on program impacts across the 16 study sites, which allow us to consider whether the local JTPA programs differed in impacts and to identify local program characteristics that led to the largest impacts.

We do not present estimates of the effects of JTPA on all target groups combined precisely because of the important differences in the program impacts between adults and youths and, to a lesser extent, between females and males. Indeed, the differences are so fundamental as to render estimates of average program impact on the combined sample essentially meaningless. The compare-and-contrast approach followed here therefore seems the best way to tell the story of the program's overall effectiveness at the study sites.

The chapter begins by examining the effects of JTPA on earnings and employment in each of the four target groups. It then proceeds to compare the experiences of the three service strategy subgroups in each target group—the classroom training, OJT/JSA, and other services subgroups—examining for each the employment and training services received and the impact of those services on earnings and the two main components of earnings (hours worked per assignee and earnings per hour worked). Later sections of the chapter examine impacts on key subgroups (such as those facing the employment barrier of welfare receipt or limited education) within each target group and variations in impacts across sites.

The comparisons presented here are both statistical and descriptive in nature, making the analysis as a whole somewhat more tentative than that of the three preceding chapters. Only the most important comparisons across target groups and study sites are tested for statistical significance, in part because comparisons between target groups or sites have less power to detect real differences than tests of each target group individually.[1] Hence, one should not be surprised that only a few of these comparisons yielded statistically significant results.

Impacts on Earnings and Employment Overall

Exhibit 7.1 shows the estimated effects of JTPA on earnings, the percentage employed, and the average number of weeks and hours worked per assignee for each of the four target groups over the full 18-month follow-up period. For each target group the exhibit displays both the mean outcome for the control group and the impact per JTPA assignee. The *impact per assignee* (treatment group member) is defined as the difference between the treatment group mean and the control group mean. Recall from earlier chapters that these estimates reflect the *average* effects of access to JTPA on all assignees, or more specifically, how much higher (or lower) earnings and employment levels were for those who had access to the program than for those who did not.

Also note that all of the impact estimates in the exhibit refer to JTPA assignees overall—whether they became enrolled in JTPA or not. As explained in earlier chapters, the inferred impact estimates for the narrower population of assignees who did become enrolled were uniformly larger than those shown here for assignees, if one assumes that the nonenrollees experienced no program effects. For simplicity we confine our attention in this chapter to impacts per assignee. The inferred *impacts per enrollee* (under the assumption of no effects on nonenrollees) that appeared in chapters 4 through 6 exceeded the impacts per assignee by 59 percent to 71 percent, depending on the target group. Thus, comparisons across target groups would change only slightly if we used the per enrollee estimates instead of the per assignee estimates.

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[1] Tests of one target group against another are subject to greater sampling variability than tests of a single target group, since they are based on two sample-based estimates instead of one. The same is true of tests comparing different sites which are also limited by the small samples available from each site.

IMPACTS ON EARNINGS: JTPA ASSIGNEES OVERALL

Because the four target groups differed substantially in their baseline characteristics (see exhibits 3.13 and 3.14 in Chapter 3), we would expect control group earnings over the 18-month follow-up period to vary considerably by target group. As shown in the first row of Exhibit 7.1, the male control groups earned much more than their female counterparts, and the adult control groups earned somewhat more than their youth counterparts. It is these findings for the control groups that represent our estimates of what JTPA assignees would have earned without access to the program.

The four target groups also differed somewhat in the JTPA services they were recommended for and received, as will be shown later in the chapter. Together the differences in individual characteristics and services recommended and received led to substantial differences in the estimated program impacts on earnings for the four target groups. At one extreme is the positive and statistically significant impact of $539 on adult women's earnings (7 percent of the control group mean). The estimated impact on adult men was similar, at $550, although that figure was not statistically significant at conventional levels and represented a smaller percentage (4.5 percent) of the control group mean.[2] At the other extreme is the negative and statistically significant impact of $-854 on male youths' earnings (or -8 percent of the control group mean)—a finding that stems mainly from negative impacts on male youths with a previous arrest and that is statistically significantly different from the positive estimated impacts on both of the adult target groups.[3] Female youths seemed to experience little or no program effect on earnings, a finding that again is significantly different from the results for both of the adult target groups. Impacts did not differ significantly by gender for either adults or youths.

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[2] As discussed in Chapter 5, despite this lack of statistical significance, the estimated impact on adult men is likely to reflect a true impact, for several reasons. First, the estimated impacts on adult men's earnings were positive in all six follow-up quarters and for all three service strategy subgroups. Second, the $550 impact estimate for the full 18-month period was statistically significant at a near-conventional level (.15). Finally, and most tellingly, the estimated impacts on all three measures of employment (the percentage employed and the number of weeks and hours worked) for adult men were positive *and* statistically significant.

[3] Recall from Chapter 6 that the extremely negative survey-based estimate of the impact on the earnings of male youths with a previous arrest—and thus the very negative estimated impact on earnings for male youths overall—is not supported by impact estimates based on an alternative data source, namely, earnings data from state unemployment insurance agencies. The survey-based estimates presented here should therefore be viewed with caution until further analyses appear in our forthcoming final report.

Exhibit 7.1 *Impacts on Total 18-Month Earnings and Employment: JTPA Assignees, by Target Group*

	Adult women		*Adult men*		*Female youths*[a]		*Male youths*[a]	
Impact on:	*Control mean (1)*	*Impact per assignee (2)*	*Control mean (3)*	*Impact per assignee (4)*	*Control mean (5)*	*Impact per assignee (6)*	*Control mean (7)*	*Impact per assignee (8)*
Earnings, in $	$ 7,488	$ 539***	$ 12,306	$ 550	$ 6,225	$ -182	$ 10,736	$ -854**
% employed,[b] in % points	76.8%	2.1%**	83.6%	2.8%**	78.1%	2.8%	89.0%	1.5%
Weeks worked, in weeks	38.3	1.1	45.3	2.2**	34.7	-1.6	45.7	-2.3*
Hours worked, in hours	1,403	52	1,865	84**	1,302	-61	1,892	-129**
Sample size[c]		*6,474*		*4,419*		*2,300*		*1,748*

Sources: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state unemployment insurance (UI) agencies. See exhibits 4.4, 4.6, 5.3, 5.5, and 6.2 in chapters 4, 5, and 6, and H. 3 and H. 9 in Appendix H.

Note: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. Out-of-school youths only.

b. At any time during the 18-month follow-up period.

c. Treatment and control groups combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

IMPACTS ON EMPLOYMENT: JTPA ASSIGNEES OVERALL

The remaining rows of Exhibit 7.1 display the estimated effects of JTPA on three different employment outcomes: the percentage of assignees employed at any time during the 18-month follow-up period and the average number of weeks and hours worked during that period.[4] Again there are substantial differences in impacts between the adults and the youths.

Adult women in JTPA experienced a significant increase in overall employment, up 2.1 percentage points from the control group mean of 76.8 percent. Estimated increases in weeks and hours worked were of a similar magnitude (not shown, but around 3 percent of the corresponding group control mean) but not statistically significant. For adult men the three estimates of impacts on employment were slightly higher, and all three were statistically significant. JTPA increased overall adult male employment by an estimated 2.8 percentage points and resulted in 2.2 more weeks and 84 more hours of employment over the follow-up period. Each of these effects represents a 3 percent to 5 percent increase over the corresponding control group mean (not shown)—again, similar to or slightly larger than the estimated effects for adult women.

On the other hand, no statistically significant increases in employment are evident among the youths, although the estimated impact on the percentage employed was insignificantly positive for both females and males. The estimated effects on weeks and hours worked were negative for both youth target groups, with those for the males statistically significant and of a greater magnitude than those for the females. Specifically, these estimates show a drop in work time of 2.3 weeks and 129 hours for male youths, or 5 percent to 7 percent of the corresponding control group mean (not shown). For both genders the estimated effects on weeks and hours worked were significantly more negative for youths than for adults. Estimated effects on the three employment measures did not differ significantly among target groups in any other instance.

In summary, then, these findings of impacts on employment provide a fuller picture of the differences in program impacts across target groups than simply the earlier evidence on earnings. The gains among adults have been clarified in two respects. For adult women we find that the gains in earnings were accompanied by an increase in the proportion of women who worked at some time during the 18-month follow-up period. For adult men the evidence of program benefits is much stronger once we examine the three employment outcomes, all of which were significantly increased by access to JTPA.

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[4] Both the weeks worked and the hours worked estimates in Exhibit 7.1 are averages for the group in question; that is, the estimates include zero values for those sample members who did not work at all.

Among youths we see that JTPA did not reduce the percentage of the males ever employed during the 18-month follow-up period, as it did the other labor market outcomes considered. No significant employment effects occurred for female youths.

Services Recommended and Received

These differences in labor market impacts across the target groups could have arisen from either or both of two sources: differences across target groups in JTPA services recommended and received, or differences in the ability of the various target groups to benefit from a given set of services. For example, the services adults and youths received may have differed because of differences in the JTPA performance standards, which emphasize employment for adults but also include educational attainment for youths. This study was not designed to trace differences in impacts to differences in programs versus differences in participants, but to measure the net effect of the two taken together as they naturally occur within the JTPA system. We can, however, provide some insight into the influence of service differentials by comparing subsets of individuals from each target group who were recommended for the same service strategy.

SERVICE STRATEGIES RECOMMENDED: TREATMENT GROUP

Exhibit 7.2 shows that the four target groups did, in fact, differ substantially in the mix of service strategies for which they were *recommended*.[5] Among both adult women and female youths *classroom training* was the most frequently recommended service strategy; among men it was the *OJT/JSA* strategy; and among male youths it was the *other services* strategy. For the classroom training strategy, SDA staff recommended 44 percent of all adult women and female youths, but only a quarter to under a third of male youths. The staff were much more apt to recommend adults—both women and men—for the OJT/JSA strategy, whereas the reverse was true of the other services strategy. A particularly noticeable difference was that adult men were much more likely than all three of the other target groups to be recommended for on-the-job training (the OJT/JSA strategy).

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[5] Statistical tests of these differences were not conducted, however.

Exhibit 7.2 Service Strategies Recommended: Treatment Group, by Target Group

Recommended service strategy	*Adult women (1)*	*Adult men (2)*	*Female youths[a] (3)*	*Male youths[a] (4)*
Classroom training	44.0%	24.6%	44.3%	29.9%
OJT/JSA	35.0	48.7	23.2	32.9
Other services	21.0	26.7	32.5	37.3
Sample size	*4,465*	*3,759*	*1,814*	*1,436*

Source: Unadjusted frequencies based on Background Information Form responses. See Exhibit 3.16.

a. Out-of-school youths only.

JTPA SERVICES RECEIVED: TREATMENT GROUP, BY SERVICE STRATEGY SUBGROUP

As shown in the three preceding chapters, the service strategy recommendations of program staff resulted, in turn, in subgroups of JTPA enrollees who *received* distinctly different clusters of specific program services. Moreover, because members of each service strategy subgroup could receive more than one service, a given subgroup could differ substantially across target groups in terms of the mix of specific program services received. Thus, a comparison of differences in the mix of services received across target groups within each service strategy subgroup will lend insight into our later presentation of program impacts by service strategy subgroup.

Exhibit 7.3 shows the percentage of treatment group members who received various JTPA services during the 18-month follow-up period, by target group and service strategy subgroup.[6] Differences within each service strategy subgroup were modest but potentially important.[7]

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[6] Percentages sum to more than 100 percent within columns because sample members could receive more that one employment and training service. As explained in earlier chapters, most treatment group members who failed to receive one of the key services in their service strategy subgroup did so because they were never enrolled in JTPA. Thus, service receipt rates among JTPA *enrollees* were substantially higher than those shown in Exhibit 7.3. For the most common service received in each service strategy subgroup these rates of receipt *per service receipt* ranged from 70 percent to 79 percent in the classroom training subgroup (for classroom training in occupational skills), from 50 percent to 52 percent in the OJT/JSA subgroup (for on-the-job training), and from 48 percent to 52 percent in the other services subgroup (for miscellaneous services).

[7] These differences were *not* tested for statistical significance.

Exhibit 7.3 *Receipt of Specific JTPA Services: Treatment Group, by Target Group and Service Strategy Subgroup*

	% of treatment group receiving the service			
Specific program service	*Adult women (1)*	*Adult men (2)*	*Female youths* [a] *(3)*	*Male youths* [a] *(4)*
		Classroom training subgroup		
Never enrolled	27.2%	28.8%	28.5%	25.2%
Classroom training in occupational skills	57.8	55.7	54.8	52.4
Basic education [b]	10.6	8.8	17.8	23.3
On-the-job training	3.3	5.4	2.6	4.4
Job search assistance	17.1	12.4	27.3	30.8
Work experience	3.9	1.7	5.7	6.5
Miscellaneous [c]	11.3	9.7	7.7	7.9
		OJT/JSA subgroup		
Never enrolled	44.6%	43.4%	42.5%	41.5%
Classroom training in occupational skills	5.1	2.1	3.3	1.9
Basic education [b]	2.6	3.6	3.1	2.8
On-the-job training	28.5	26.6	29.9	30.5
Job search assistance	26.5	30.2	28.3	32.2
Work experience	2.6	2.4	5.2	4.2
Miscellaneous [c]	5.8	6.8	7.1	6.8
		Other services subgroup		
Never enrolled	37.6%	41.1%	36.9%	32.3%
Classroom training in occupational skills	15.6	4.9	9.8	6.5
Basic education [b]	11.1	6.1	29.7	26.4
On-the-job training	5.5	4.7	3.9	3.9
Job search assistance	23.4	24.8	12.2	12.0
Work experience	2.7	0.9	3.4	3.2
Miscellaneous [c]	31.5	28.4	28.5	35.3
Sample size	*4,465*	*3,759*	*1,814*	*1,436*

Source: Enrollment and tracking data from the 16 service delivery areas (SDAs). See Exhibit 3.18.

[a] Out-of-school youths only.

[b] "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

[c] "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

Within the *classroom training subgroup* around 55 percent of the treatment group in all four target groups received the defining service for the service strategy—classroom training in occupational skills. Treatment group members in the youth target groups were, however, much more likely to receive basic education and job search assistance than their counterparts in the adult target groups—with 20 percent of youths versus around 10 percent of adults enrolled in basic education courses and around 30 percent of youths versus around 15 percent of adults enrolled in job search assistance.

No important differences in service receipt across target groups occurred for those in the *OJT/JSA subgroup*. For all four target groups enrollment in each of the two key services in the strategy—on-the-job training and job search assistance—was around 30 percent.

The largest contrasts occurred within the *other services subgroup*, which, for adults, was characterized by receipt of miscellaneous services and job search assistance and, for youths, by receipt of miscellaneous services and basic education. Specifically, the adults (at around 24 percent) were more likely than the youths (at 12 percent) to receive job search assistance (which is the opposite of the results for job search assistance in the classroom training subgroup). The youths, on the other hand, were much more likely (at around 28 percent) than the adults (between 6 and 11 percent) to receive basic education. Even the service category that had the highest enrollment rates in all four target groups showed some variation. About 28 percent of adult men and female youths received miscellaneous services, whereas the corresponding figures for adult women and male youths were 32 percent and 35 percent. And adult women in the other services subgroup were much more apt to become enrolled in classroom training in occupational skills than any other target group (16 percent versus a range of 5 to 10 percent).

Finally, within each service strategy subgroup differences in overall enrollment in JTPA and in the average duration of enrollments were slight. As shown in the first row of each panel in Exhibit 7.3, the percentages of the treatment group who were never enrolled in JTPA over the 18-month follow-up period were very similar across target groups in both the classroom training subgroup and the OJT/JSA subgroup. In the other services subgroup the female target groups had virtually the same rate of nonenrollment (about 37 percent), but adult men were more likely (41 percent) and male youths less likely (32 percent) to never enroll. And as shown earlier (Exhibit 3.20 in Chapter 3), the average durations of program enrollments within each service strategy subgroup were also quite similar across target groups, with the minor exceptions of slightly shorter average enrollment rates among the males than the females in the classroom training and other services subgroups.[8]

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[8] These differences were not tested for statistical significance.

All told, then, the only substantial differences in services received and enrollment within each service strategy subgroup again distinguish the youths from the adults. In the classroom training subgroup the youths were much more likely to receive basic education and job search assistance, and in the other services subgroup the youths were apt to receive more basic education but less job search assistance than the adults.

SERVICES RECEIVED FROM ANY PROVIDER: TREATMENT AND CONTROL GROUPS, BY SERVICE STRATEGY SUBGROUP

Although informative, the above comparisons of JTPA services recommended and received across target groups do not present a complete picture of the differences in service receipt that created the labor market impacts shown earlier. Bear in mind that those impacts were produced by differences in service receipt between the treatment group and control group members within each target group–service strategy subgroup combination—differences that go beyond those measured by the JTPA services received by treatment group members alone. Control group members also received employment and training services during the follow-up period, from non-JTPA sources, as did some members of the treatment group.

Exhibit 7.4 summarizes what we know about *treatment-control group differences* in service receipt from any provider, by target group within each service strategy subgroup. The exhibit focuses on the average *amount* of each service received (in hours, including zero hours for those who received no services) as the best overall measure of service receipt.[9] As explained in Chapter 4, we were unable to obtain reliable data on the receipt of job search assistance and miscellaneous services from non-JTPA service providers. Nor were we able to obtain data on the receipt of on-the-job training and work experience from non-JTPA providers, although in these instances we are fairly confident the JTPA-only data shown in the exhibit represent nearly all of the hours of service received, since on-the-job training and work experience are seldom available from other providers.

The differences in the average number of hours of service receipt shown in Exhibit 7.4 are diluted by the high proportion of both treatment and control group members who received zero hours of each type of service. Hence, they appear small even though they may represent substantial additional amounts of services for *service recipients* within the treatment and control groups.

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[9] Additional details on the *likelihood* (percentage) of the treatment and control groups receiving each service, and on the number of hours of each service received by the treatment and control groups separately, appeared earlier in exhibits 4.8, 5.7, 6.4 and 6.9.

Exhibit 7.4 *Difference in the Mean Number of Hours of Employment and Training Services Received by the Treatment Group and Control Group: Target Groups, by Service Strategy Subgroup*

	Treatment-control group difference			
Specific program service	*Adult women (1)*	*Adult men (2)*	*Female youths* [a] *(3)*	*Male youths* [a] *(4)*
	Classroom training subgroup			
Classroom training in occupational skills [b] +	110	95	187	127
Basic education [c] +	9	9	36	16
On-the-job training [d] ++	25	37	24	18
Work experience [d] ++	23	8	32	25
Job search assistance +++	--	--	--	--
Miscellaneous [e] +++	--	--	--	--
Sample size [f]	*2,847*	*1,057*	*1,045*	*526*
	OJT/JSA subgroup			
Classroom training in occupational skills [b] +	-2	3	-16	58
Basic education [c] +	7	-2	-2	-32
On-the-job training [d] ++	104	114	105	128
Work experience [d] ++	13	10	17	10
Job search assistance +++	--	--	--	--
Miscellaneous [e] +++	--	--	--	--
Sample size [f]	*2,287*	*2,250*	*545*	*615*

(Continued)

Even with this dilution, youth treatment group members in the *classroom training subgroup*—especially female youths—held a noticeable advantage over their control group counterparts in hours of classroom training in occupational skills: differences of 187 hours for females and 127 hours for males. The only clear treatment-control group difference in hours of basic education received—36 hours—also occurred within the female youth target group. Both of these differences for female youths represent a doubling of the average amount of the services received by the corresponding members of the control group, though not a large gain in terms of the total number of hours of service receipt.[10] No other target group received so large an increment in services when recommended for classroom training, in either hourly or percentage terms.[11]

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[10] Among *service recipients only* (roughly two-thirds of the female youth treatment group), the difference in the average amount of classroom instruction of either type that was received was substantially larger, though still modest: 330 total hours of classroom training in occupational skills and basic education, rather than the combined total of 223 hours shown in Exhibit 7.4.

[11] No tests were performed to determine if the increment in services received by female youth treatment group members in the classroom training subgroup significantly exceeded that of the classroom training subgroup in the other three target groups.

Exhibit 7.4 Difference in the Mean Number of Hours of Employment and Training Services Received by the Treatment Group and Control Group: Target Groups, by Service Strategy Subgroup (continued)

	Treatment-control group difference			
Specific program service	*Adult women (1)*	*Adult men (2)*	*Female youths*[a] *(3)*	*Male youths*[a] *(4)*
		Other services subgroup		
Classroom training in occupational skills[b] +	18	-4	1	10
Basic education[c] +	7	8	3	-1
On-the-job training[d] + +	35	27	21	14
Work experience[d] + +	18	6	3	8
Job search assistance+ + +	--	--	--	--
Miscellaneous[e] + + +	--	--	--	--
Sample size[f]	*1,340*	*1,112*	*710*	*607*

Sources: See exhibits 4.8, 5.7, 6.4, and 6.9.

+ The differences in mean hours in this row are based on First Follow-up Survey data on receipt of the service from any provider.

+ + The differences in mean hours in this row are based on enrollment and tracking data from the 16 SDAs, the best available data on receipt of this service. Although the data are for JTPA Title II-A-funded services only, this service is typically not funded by non-JTPA providers.

+ + + No estimates are reported in this row because data were not available on receipt of this service from other providers or on receipt by control group members.

Notes: Because of missing data, sample sizes for services calculated from different data sources may vary. Tests of statistical significance were not performed for this exhibit.

a. Out-of-school youths only.

b. Lasting longer than one week.

c. Lasting longer than one week. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

d. Hours, assuming a full-time job at 40 hours per week.

e. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

f. Treatment and control groups combined.

Treatment-control group differences in service receipt are not so easily assessed for the key services in the other two service strategy subgroups. Data on service receipt from non-JTPA providers are unavailable for job search assistance (a key service in the OJT/JSA subgroup and, for adults, in the other services subgroup) and for miscellaneous services (a key service in the other services subgroup). For the *OJT/JSA subgroup* we can at least compare the treatment group's advantage in the amount of on-the-job training received across target groups, on the assumption that OJT is available only through JTPA. That comparison reveals an almost uniform treatment group advantage ranging from 104 to 128 hours across target groups.[12] This lack of variation across target groups is consistent with the earlier finding that the OJT/JSA service strategy subgroup varied least across target groups in terms of *JTPA* services received. The variation in treatment-control group differences in the receipt of basic education—a key service for youths in the *other services subgroup*—was also slight between female and male youths.

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[12] Among *service recipients only* the difference in the average amount of OJT received was nearly four times as great. The differences shown here combine a difference of around 400 hours per service recipient for the 27 percent to 30 percent of OJT/JSA treatment group members who actually became enrolled in OJT with a difference of 0 hours for the remaining 70 percent to 73 percent

Thus, in general, the increments in the amount of service receipt attributable to access to JTPA were modest and quite similar across the target groups, although noticeable differences did arise in the classroom training subgroup.

HIGH SCHOOL ATTAINMENT: TREATMENT AND CONTROL GROUPS

As part of the school or training services listed in Exhibit 7.4, a number of sample members gained a high school diploma or GED certificate. Exhibit 7.5 shows the percentages of the treatment and control group members in each target group–service strategy subgroup combination who both participated in school or training and achieved a high school credential during the follow-up period. We refer to this outcome as the *attainment of a training-related high school credential*. For simplicity we confine our attention here to attainment rates for the treatment group overall rather than considering the rates for both treatment group members and the high school dropout subgroup presented in the three earlier chapters.

Not surprisingly given their advantage in receipt of basic education services, youths recommended for the *classroom training* and *other services strategies* were substantially more likely to obtain a training-related high school credential than adults recommended for those strategies. Fifteen to 19 percent of youth treatment group members in the first and third service strategies received a training-related high school credential, whereas only 3 to 7 percent of their adult counterparts did. These findings reflect in part the substantially larger proportion of adults than youths who already had a high school credential when they applied to JTPA (see exhibits 3.13 and 3.14 in Chapter 3).

Treatment-control group differences in attainment for the classroom training and other services subgroups were also larger for youths than adults, but were positive and statistically significant in all four target groups (not shown).[13] No important differences by gender appear in either the classroom training or other services subgroup.

Adults and youths recommended for the *OJT/JSA strategy* differed little in attainment of a training-related high school credential, even though a substantially greater share of

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[13] For adult men within the other services subgroup the treatment-control group difference in high school attainment was positive but not statistically significant. Comparisons of treatment-control group differences in attainment between pairs of target groups were not tested for statistical significance

Exhibit 7.5 *Attainment of a Training-Related High School Diploma or GED Certificate: Treatment Group and Control Group, by Target Group and Service Strategy Subgroup*

	Adult women		*Adult men*		*Female youths*[a]		*Male youths*[a]	
	Treatment (1)	*Control (2)*	*Treatment (3)*	*Control (4)*	*Treatment (5)*	*Control (6)*	*Treatment (7)*	*Control (8)*
				Classroom training subgroup				
Percentage attaining training-related H. S. credential	7.3%	2.9%	6.7%	2.2%	15.4%	7.6%	16.6%	10.3%
Sample size	*1,606*	*784*	*699*	*319*	*675*	*327*	*344*	*165*
				OJT/JSA subgroup				
Percentage attaining training-related H. S. credential	2.6%	3.5%	2.6%	1.5%	3.4%	1.9%	5.8%	2.0%
Sample size	*1,320*	*635*	*1,435*	*689*	*357*	*159*	*395*	*200*
				Other services subgroup				
Percentage attaining training-related H. S. credential	5.3%	3.3%	3.4%	2.8%	19.0%	13.1%	18.7%	11.8%
Sample size	*729*	*335*	*680*	*364*	*480*	*199*	*418*	*170*

Sources: Unadjusted frequencies based on Background Information Form responses and First Follow-up Survey responses. See exhibits 4.9, 5.8, 6.5, and 6.10.

Note: "Attainment of a training-related high school credential" is defined as the combination of having received some school or training service and having attained a high school diploma or General Education Development certificate at some time during the 18-month follow-up period.

[a] Out-of-school youths only.

youths than adults could have gained a high school diploma or GED certificate during the follow-up period (that is, a higher percentage of youths lacked a high school credential at baseline). Attainment rates were small (2 to 6 percent) for all target groups in the OJT/JSA subgroup.[14]

Impacts on Earnings across Service Strategy Subgroups

With these service and educational differentials in mind, we turn now to a comparison of JTPA impacts across target groups in each of the service strategy subgroups. This comparison offers further insight into differential program effects on the target groups by focusing on sample members recommended for a similar mix of services. This focus, in turn, provides a better understanding of how differences in the personal characteristics of the four target groups led to different impacts on earnings. Remember, though, that even within a given service strategy subgroup (such as classroom training or other services) patterns of service receipt did vary somewhat by target group.

IMPACTS ON 18-MONTH EARNINGS

We begin by focusing on variations among the target groups in 18-month impacts on earnings for assignees in the same service strategy subgroup. The "all quarters" rows of Exhibit 7.6 display these findings. Reflecting the general differences in baseline characteristics among the target groups as a whole, *control group earnings* over the full follow-up period differed across target group–service strategy subgroup combinations. Males earned significantly more than females, and in most cases adults earned significantly more than youths. The male-female contrast is particularly striking, with male earnings ranging from $9,783 to $12,765 and female earnings ranging from $5,726 to $8,607 in all three service strategy subgroups.

Program *impacts* also vary in a highly regular fashion across target group-service strategy subgroup combinations, with the impacts for adults more positive than those for youths in all cases. This contrast is often pronounced in its dollar magnitude, although usually not statistically significant.[15]

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[14] Cross-target group comparisons of high school attainment rates and impacts for *high school dropouts only* show greater variation (see exhibits 4.9, 5.8, 6.5, and 6.10) than the findings for the treatment group as a whole reported in this subsection. We report the attainment findings here for the full treatment group (including those who *had* a high school credential when they applied to JTPA) to focus on attainment of a high school credential as an intermediate step toward achieving the ultimate goal of JTPA: higher earnings for all program participants, in this case treatment group members overall.

[15] Large differences between target groups are less likely to yield statistically significant differences in estimates for the individual service strategy subgroups because of the much smaller samples sizes involved (approximately one-third the sample sizes used in the original comparison of the full target groups).

Exhibit 7.6 Impacts on Quarterly and 18-Month Earnings: JTPA Assignees, by Target Group and Service Strategy Subgroup

	Adult women		Adult men		Female youths[a]		Male youths[a]	
Period	Control mean (1)	Impact per assignee (2)	Control mean (3)	Impact per assignee (4)	Control mean (5)	Impact per assignee (6)	Control mean (7)	Impact per assignee (8)
Classroom training subgroup								
Quarter 1	$ 714	$ -70*	$ 1,440	$ -101	$ 742	$ -210***	$ 1,226	$ -300**
2	938	5	1,714	126	909	-189***	1,345	96
3	1,066	52	1,884	213	1,052	-150*	1,655	-2
4	1,189	79	2,184	50	991	24	1,773	0
5	1,253	144**	2,171	151	1,047	70	1,889	-56
6	1,230	188***	2,387	-21	1,196	-87	1,895	4
All quarters	6,391	398	11,780	418	5,936	-542	9,783	-259
Sample size[b]		*2,847*		*1,057*		*1,045*		*526*
OJT/JSA subgroup								
Quarter 1	$ 1,143	$ 144***	$ 1,757	$ 54	$ 1,002	$ 149	$ 1,651	$ -57
2	1,379	81	2,014	135	1,074	203*	1,988	-219
3	1,449	129**	2,133	164*	1,252	97	2,197	-302*
4	1,520	109*	2,199	94	1,363	3	2,160	-203
5	1,546	142**	2,183	133	1,368	103	2,316	-192
6	1,570	138**	2,169	201**	1,562	-146	2,452	-339**
All quarters	8,607	742**	12,456	781*	7,620	410	12,765	-1,313*
Sample size[b]		*2,287*		*2,250*		*545*		*615*
Other services subgroup								
Quarter 1	$ 960	$ 39	$ 1,677	$ 74	$ 653	$ 43	$ 1,362	$ -285**
2	1,198	132	1,951	104	909	-68	1,457	-121
3	1,248	220**	2,123	44	1,023	-96	1,605	-218
4	1,471	22	2,199	44	1,047	-52	1,751	-276*
5	1,535	2	2,292	13	1,093	-41	1,766	-114
6	1,548	42	2,274	-19	1,001	55	1,899	-292**
All quarters	7,960	457	12,516	261	5,726	-158	9,839	-1,305*
Sample size[b]		*1,340*		*1,112*		*710*		*607*

Sources: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state UI agencies. See Exhibits 4.12, 5.11, 6.7, and 6.12.

Note: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. Out-of-school youths only.

b. Treatment and control groups combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

For the *classroom training subgroup*, estimated impacts on 18-month earnings were positive and statistically insignificant for adults, and negative and insignificant for youths. Differences between target groups were not statistically significant, except in the case of the two extremes: the $418 positive estimate for adult women and the $-542 negative estimate for female youths.

The contrasts are much sharper in the *OJT/JSA subgroup*, where most of the statistically significant impacts on 18-month earnings by service strategy occurred. Here we find estimated earnings gains of $742 for adult women and $781 for adult men and an estimated $-1,313 loss for male youths (which reflects the extremely negative effects on male youths with a previous arrest record as measured by the survey data; see Chapter 6).[16] The estimate for female youths was positive, at $410, but not statistically significant. Despite this wide range, only the estimate for male youths differed significantly from those for the other target groups.

The *other services subgroup* of male youths experienced a similar significant earnings loss of $-1,305 (again largely due to negative effects on male youths with a previous arrest). Members of the other services subgroup in the other three target groups did not experience a significant impact on earnings. The only statistically significant difference in effects among these subgroups was between the estimated gain of $457 for adult women and the estimated loss of $-1,305 for male youths.

IMPACTS ON QUARTERLY EARNINGS

In addition to these effects on 18-month earnings, Exhibit 7.6 provides information on the time path of earnings and earnings effects within each service strategy and target group. We do not present this breakdown into three-month periods, or quarters, for the target groups as a whole, since the findings in chapters 4, 5, and 6 clearly indicate that the time paths of the impacts differed substantially across service strategies. We consider instead whether those time paths also differed by target group within each service strategy subgroup. The discussion is limited, however, by the fact that statistical tests for trends in effects over time or for differences in trends across target groups have not been run.

As shown in the first column for each target group, control group earnings generally rose steadily over time in all four target groups irrespective of service strategy. This pattern—particularly pronounced for males—reflects the tendency to apply for employment and training assistance from JTPA when earnings are unusually low.

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[16] These earnings gains in the OJT/JSA subgroup stem in part from the subsidized wages paid to JTPA participants in OJT positions, which averaged between $150 and $190 per *OJT participant* in each target group over the 18-month follow-up period.

In the *classroom training subgroup* (top panel of Exhibit 7.6) the most distinct trend in impacts on earnings is that for adult women. In fact, column 2 displays precisely what one would expect of a strategy that initially diverts participants from employment to train them so that they may earn more later: significant earnings losses such as those in the first quarter after random assignment that turned into significant earnings gains by the last two quarters of the follow-up period. But the time trends for the classroom training subgroups of the other three target groups were not nearly so clear. Youths experienced significant earnings losses in the first one to three quarters but no material gains in later quarters, while adult men experienced no consistent trend over time.

Impacts in the *OJT/JSA subgroup* also followed no obvious trend, although they were consistently (and sometimes significantly) positive in all quarters for adults and in most quarters for female youths. For male youths, however, the OJT/JSA service strategy produced consistently large, negative impacts beginning in the second quarter after random assignment, with estimated losses that were statistically significant in two of the six quarters. Because differences in impacts between male youths and the other target groups were fairly stable over time, a comparison of the *trends* in impacts reveals no noticeable differences across target groups. In other words, the OJT/JSA service strategy was either almost steadily beneficial or—in the case of male youths with a previous arrest—steadily and markedly negative in its earnings effects.

Like the OJT/JSA subgroup, none of the target groups in the *other services subgroup* experienced a distinct trend in impacts on earnings over time. There is, however, a hint that this service strategy became less effective over time for the adults, especially adult women, who seem to have experienced declining gains after the third quarter after random assignment. Throughout the follow-up period, female youths appear to have experienced negligible impacts, and male youths, markedly negative impacts.

GENERAL FINDINGS

To summarize, JTPA had both its strongest impacts (both favorable and unfavorable) and its most varied impacts across target groups within the *OJT/JSA service strategy*. Differences in service receipt across target groups were the *least* evident for this service strategy as measured by the types of JTPA services received, hours of services received relative to the control group, and attainment of a high school credential (again, relative to the control group). Substantially greater variation in these factors across target groups within the other two service strategies did not produce such striking variations in impact, suggesting that variations in impact were not produced by differences in the factors considered here. Other potentially important factors may have played a role, however, such as the quality and content of the assistance received or the ability of the service recipients in the OJT/JSA subgroup to benefit from the services they received.[17]

With one exception noted below, we cannot be sure that either the classroom training or other service strategies significantly affected earnings for any target group over the full 18-month follow-up period. We do know, however, that the *classroom training strategy* reduced earnings initially for at least three of the four target groups, with the loss for adult women offset to some degree by gains later in the follow-up period.

The key exception to the generally negligible impact findings for the first and third service strategies is the finding for male youths in the *other services subgroup*: a substantial loss in earnings that actually surpassed, in percentage terms, the loss sustained by male youths in the OJT/JSA subgroup. This loss differed significantly from the effect of JTPA on the other services subgroup of adult women who, in fact, seemed to benefit from the strategy early in the follow-up period. Whether this difference should be attributed to a different mix of services—relatively more job search assistance for adult women, relatively more basic education and high school attainment for male youths—is unclear. It seems unlikely that this distinction was decisive, however, since the same service contrast between other pairs of adult and youth target groups did not lead to other similarly significant variations in impact.

Taken as a whole, this comparison of earnings and impact patterns across target groups is remarkable in its regularity, at least within the range of variation that our data were able to detect with confidence.

Impacts on the Components of Earnings

Variations in earnings impacts across target groups necessarily stemmed from impacts on one or more of the four components of earnings measured in chapters 4 through 6: employment rates, weeks worked if employed, hours worked per week worked, and earnings per hour worked.

Exhibit 7.7 shows a simplified version of the component estimates that appeared in earlier chapters. Impacts on the percentage employed, weeks worked if employed, and hours worked per week worked are combined here into a single measure—percentage effects on hours worked across all sample members and weeks. This component of the

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[17] Recall that of all four target groups the OJT/JSA subgroup was by far the most employable of the three service strategy subgroups, as measured by the estimated mean earnings of the control group over the 18-month follow-up period.

Exhibit 7.7 *Percentage Impacts on Total 18-Month Earnings and Selected Earnings Components: JTPA Assignees, by Target Group and Service Strategy Subgroup*

Percentage impact on:	*Adult women (1)*	*Adult men (2)*	*Female youths [a] (3)*	*Male youths [a] (4)*
Classroom training subgroup				
Earnings per assignee	6.2%	3.5%	-9.1%	-2.6%
Hours worked per assignee	-2.5	4.2	-9.7*	-2.9
Earnings per hour worked	8.9	-0.6	0.6	0.3
Sample size [b]	*2,847*	*1,057*	*1,045*	*526*
OJT/JSA subgroup				
Earnings per assignee	8.6%**	6.3%*	5.4%	-10.3%*
Hours worked per assignee	5.9*	6.3*	-0.8	-8.3
Earnings per hour worked	2.6	0.0	6.2	-2.1
Sample size [b]	*2,287*	*2,250*	*545*	*615*
Other services subgroup				
Earnings per assignee	5.7%	2.1%	-2.8%	-13.3%*
Hours worked per assignee	5.7	1.7	-0.4	-9.6
Earnings per hour worked	0.0	0.4	-2.4	-4.0
Sample size [b]	*1,340*	*1,112*	*710*	*607*

Sources: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state UI agencies. See exhibits 4.14, 4.16, 5.13, 5.15, 6.8, 6.13, and 6.16.

Notes: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. Out-of-school youths only.

b. Treatment and control groups combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Tests of statistical significance were not performed for impacts on earnings per hour worked.

overall impact on earnings captures the employment effects of the program, as distinct from its effects on earnings per hour worked, which are also shown in the exhibit. Because we did not test for the statistical significance of either this latter component or the variation in impact across target groups, these estimates must be interpreted with great care.

We focus first on the service strategy subgroup with the most obvious variations in impacts: the OJT/JSA subgroup in the middle panel of the exhibit. Here, as in the other panels, the first row gives the percentage effects on 18-month earnings, followed in the next two rows by impacts on its two components, also in percentage terms.[18] Recall from Exhibit 7.6 that, for this service strategy, the estimated effects on earnings were substantially positive for the first three target groups, and statistically significant for adult women and men. Estimates for male youths, however, were large, negative, and statistically significant.

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[18] As explained in earlier chapters, the overall percentage impact on earnings does not exactly equal the sum of the percentage impacts on its components. Close equivalence is evident in all cases, however, so that the individual percentages can be interpreted as the additive decomposition of the overall earnings effect into its component parts.

As shown in Exhibit 7.7, the significant earnings gains for adult women recommended for OJT/JSA came primarily from a 5.9 percent increase in hours worked, on average, although a 2.6 percent increase in average earnings per hour worked also played a role. For adult men the OJT/JSA strategy significantly increased earnings through a similar 6.3 percent gain in hours worked, but did not seem to increase earnings per hour worked. In general, then, improvements in the labor market outcomes of adults in the OJT/JSA subgroup resulted from more hours of work over the 18-month follow-up period rather than from higher earnings per hour.

A very different pattern emerges from the youth findings. Among female youths in the OJT/JSA subgroup the program's effect on earnings was almost entirely due to a 6.2 percent increase in earnings per hour worked. That gain does not imply, however, that JTPA necessarily made female youths more productive. It may instead reflect a program effect on the subgroup of those who were employed if, for example, additional employment was concentrated among female youths with high hourly earnings.[19]

Among male youths in the OJT/JSA subgroup a -2.1 percent drop in earnings per hour was accompanied by an 8.3 percent drop in hours worked. Together, these two factors produced a large (-10.3 percent) and statistically significant decline in earnings over 18 months.

With the male youth other services subgroup excepted, the other two service strategies—classroom training and other services—did not produce statistically significant impacts on 18-month earnings, and so the estimates in the top and bottom panels of Exhibit 7.7 are less interesting. In the *classroom training subgroup* the largest percentage impacts were those on the hourly earnings of adult women who worked (up 8.9 percent) and the hours worked by female youths (down -9.7 percent). The first finding may be important, since it suggests that adult women may have increased their hourly earnings potential through additional classroom training.[20] Sizable percentage effects also appeared twice within the other services subgroup: a 5.7 percent increase in the hours worked by adult women, and a 9.6 percent decrease in hours worked by male youths, but neither of these estimates is statistically significant.

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[19] In Appendix G we attempt to control for composition effects of this kind. Although we find no significant program effects on productivity, the analysis was nonexperimental and so its results may not be reliable.

[20] Again it is also possible that the gain in earnings per hour worked reflected a change in the composition of the subgroup of adult women who were employed. The analysis in Appendix G is not fully conclusive on this point.

If indeed they are not just chance patterns in the data, these results would suggest that access to classroom training through JTPA both (1) noticeably improved the tendency of adult women—but no other subgroup—to work in higher-paid employment, and (2) substantially diverted female youths from time they would otherwise have been spent working, without any compensating increase in their hourly earnings. The same large diversionary effect is also evident for male youths in the OJT/JSA and other services subgroups, although the OJT/JSA strategy significantly increased hours worked by adults, and potentially increased earnings per hour worked by female youths who worked. Hours worked by adult women also increased noticeably in the other services subgroup.

Impacts on Earnings across Key Subgroups

In addition to the service strategy subgroups already considered, the three preceding chapters examined the effects of JTPA Title II-A on several other subpopulations of the sample. For policy purposes the most important of these "key subgroups" that can be defined consistently across target groups are those defined by ethnicity and barriers to employment.[21]

Exhibit 7.8 displays estimated program impacts on the 18-month earnings of these subgroups in each of the four target groups. Here, rather than showing mean control group earnings, the first column for each target group indicates the relative importance of each subgroup by giving its sample size (with treatment group and control group members combined). Although the estimated impacts on quite a few subgroups were statistically significant (as indicated by asterisks beside the estimates), none of the estimates in any given set of estimates was significantly different from the other estimates in that set or panel, as indicated by "n.s." in the F-test rows.[22]

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[21] In light of our earlier findings for youths, a further subgroup of substantial policy interest is individuals who reported having been arrested between age 16 and random assignment. The information needed to identify this subgroup of the adult target groups is not available.

[22] As explained in the preceding chapters, this pattern of findings means that although we can be confident there was a significant impact on the subgroups with an asterisk beside their impact estimates, we cannot be sure whether the impacts on the other subgroups within the panel were significantly different from the estimates with the asterisk(s).

Exhibit 7.8 Impacts on Total 18-Month Earnings: JTPA Assignees, by Target Group and Selected Key Subgroup

Key subgroup, defined by:	*Impact, in dollars (sample size in parenthesis)*							
	Adult women		*Adult men*		*Female youths* [b]		*Male youths* [b]	
	(1)	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*	*(7)*	*(8)*
Ethnicity								
White, non-Hispanic	(3,541)	$ 723***	(2,668)	$ 625	(1,148)	$ -122	(946)	$ -1,333**
Black, non-Hispanic	(1,981)	457	(1,155)	957	(749)	-135	(522)	75
Hispanic	(744)	-414	(400)	-741	(366)	-554	(248)	-1,238
F-test, difference among subgroups		n.s.		n.s.		n.s.		n.s.
Barriers to employment (in italic)								
Receiving cash welfare [c]	(2,446)	387	(611)	-46	(701)	-391	(185)	-56
No cash welfare	(3,500)	697***	(3,788)	624*	(1,412)	-154	(1,374)	-1,020**
F-test, difference between subgroups		n.s.		n.s.		n.s.		n.s.
No high school diploma or GED certificate	(1,731)	416	(1,249)	398	(1,047)	23	(947)	-1,144*
High school diploma or GED certificate	(4,316)	681***	(2,873)	878**	(1,146)	-437	(730)	-420
F-test, difference between subgroups		n.s.		n.s.		n.s.		n.s.
Worked less than 13 weeks in past 12 months	(3,022)	511**	(1,614)	-210	(1,235)	-31	(754)	-1,286**
Worked 13 weeks or more in past 12 months	(2,622)	668**	(2,392)	787*	(829)	-255	(842)	-832
F-test, difference between subgroups		n.s.		n.s.		n.s.		n.s.
Number of barriers								
None of the above	(1,361)	909**	(1,465)	1,203**	(545)	-260	(475)	-459
One of the above	(1,655)	802**	(1,550)	194	(790)	-236	(733)	-695
Two of the above	(1,435)	379	(617)	30	(675)	-451	(455)	-1,242
All three of the above	(488)	-213	(116)	-146	(281)	659	(81)	-1,278
F-test, difference among subgroups		n.s.		n.s.		n.s.		n.s.

Sources: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state UI agencies. See exhibits 4.15, 5.14, 6.14, and 6.15.

Notes: Estimates are regression-adjusted to control or differences in baseline characteristics between the treatment group and control group; see Appendix D. Control means were not regression-adjusted. Sample sizes for mutually exclusive subgroups within a panel do not necessarily sum to the sample size for the target group as a whole, because persons in omitted subgroups or with missing data on the variable used to define the subgroup were excluded.

a. Treatment and control groups combined.

b. Out-of-school youths only.

c. AFDC, General Assistance, or other welfare except food stamps.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the F-test for the difference in impacts between or among the subgroups in the category is not statistically significant.

ETHNIC GROUPS

Based on the available sample sizes, we can be sure that JTPA affected the earnings of whites in two of the four target groups over the 18-month period: it increased the earnings of white adult women by $723 and reduced the earnings of white male youths by $-1,333. We saw this same pattern of results earlier for these two target groups as a whole (Exhibit 7.1), which suggests that the overall pattern of target group impacts may be largely attributable to the impacts on whites.

In light of these results, the main question to ask in looking at the impacts on the different ethnic groups is whether the cross-target group pattern for whites overshadowed the patterns of effects for the other ethnic groups. The pattern of estimated impacts for blacks and Hispanics suggests that this might have been the case, but none of the estimated effects on nonwhites was statistically significant. And although the size and signs of the estimated impacts for blacks and Hispanics often differed from those for their white counterparts, it was only within the target group of adult women that variations in effects by ethnic group were close to being statistically significant at conventional levels.[23]

Thus, the picture for the target groups overall changes only slightly when separated by ethnic group:

- Among adult women in the three main ethnic groups, white women may have been the only ones to benefit from access to JTPA.
- Black male youths may not have experienced the earnings losses that occurred for other male youths who had access to JTPA.
- No ethnic group consistently benefited from access to JTPA at a statistically significant level across all target groups.
- Hispanics in all target groups may have had their earnings reduced by JTPA, but the impact estimates on this ethnic group were not statistically significant and not significantly different from the impacts on the other main ethnic groups in each target group.

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[23] Impacts on adult women differed across ethnic subgroups at the .20 significance level, but not at the more conventional .10 level. As noted in Chapter 4, the negative estimated effects on Hispanics in this target group was concentrated in one or two sites with generally smaller impact estimates for adult women overall.

SUBGROUPS FACING SELECTED BARRIERS TO EMPLOYMENT

The second section of Exhibit 7.8 subdivides the four target groups according to the barriers to employment they were facing upon application to JTPA: welfare receipt, limited education, and limited recent work experience. Looking at the different barriers one at a time, we see first that none of the subgroups receiving cash welfare experienced statistically significant effects. Male youth high school dropouts, on the other hand, experienced a significant loss in earnings, but in the other target groups high school dropouts were not necessarily affected one way or the other. Finally, adult women who worked less than 13 of the past 52 weeks benefited from their access to JTPA, while the reverse was true for male youths in the same category. Again, however, none of these subgroup estimates differed significantly from its complement.

Because many sample members who were facing one barrier to employment were also facing another, the next panel of the exhibit divides the sample by the number, rather than the types, of barriers to employment. Here again, variations in the size of the impacts across subgroups were not statistically significant for any of the target groups. However, large positive impacts occurred for selected adult subgroups: adult women facing none or one of the barriers and adult men facing none of the barriers. No striking patterns in impacts by number of barriers occurred for the female youth or male youth target group.

In summary, the data identify no subgroups defined based on barriers to employment that show consistent evidence of beneficial effects across target groups. Nor do the estimated impacts by target group differ significantly with the presence of an individual barrier or the total number of barriers.

Impacts on Earnings across Study Sites

Up to this point all of the impact estimates presented have summarized the experiences of sample members at the 16 study sites as a whole. This section examines the sites individually to assess how much program impacts varied across localities. The next section, in turn, will investigate likely sources of the variation, namely, selected characteristics of program operations, program applicants, and the labor market in the local service delivery area (SDA). Since the study was not designed with these objectives in mind, we do not expect definitive conclusions here. Even so, anything the analysis can tell us about the variations in JTPA impacts across SDAs will add greatly to our knowledge of this important policy issue.

To increase the size of the research sample in each site, we first combine all adult women and men in one analysis and all female and male youths in another. We then consider whether patterns of impact vary by gender within each of these age groupings. For reasons of confidentiality we do not identify the SDAs by name; instead we consider them as a sample of 16 local programs whose characteristics broadly reflect those of JTPA programs nationally.[24]

The following analyses focus on the broadest measure we have of sample members' labor market success over the short term: total earnings over the 18-month follow-up period. The site-specific impact estimates parallel those presented earlier for the key subgroups of JTPA assignees.[25] The analyses in this section do not control for the possible determinants of local program impacts, which again are the subject of the next section. Instead the analyses here are designed to uncover general *patterns* of impacts that may have been produced by factors that varied from one SDA to another.

Exhibit 7.9 displays the estimated impacts, by study site, on adult and youth assignees. The impacts shown in each column are arrayed in descending order.[26] They are noteworthy in three main respects. First, the estimated impacts varied considerably for both groups: from $1,851 to $-745 for adults, and from $2,566 to $-3,591 for youths. Second, despite the broad distribution of impacts within both ranges, the impacts were positive for adults in all but three sites and negative for youths in all but four sites. Third, and as a result of these overall patterns, the estimated impact on the 18-month earnings of adults was larger than that of youths at all points on the distribution except the top.

A considerable degree of uncertainty applies to these estimates, however. Even after combining the adult women and men in one group and the female and male youth in another, sample sizes in individual sites are much smaller on average than for most of the subgroups considered elsewhere in this report.[27] Hence, chance variations in the data play a bigger role in these estimates than elsewhere. We consider the role of chance by testing whether the most striking pattern in the exhibit—large differences in estimated impacts across sites—reflects real differences in impact across sites or simply chance variations in the

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[24] See Doolittle (forthcoming) for a comparison of the study sites and SDAs nationally in terms of local environmental and program characteristics. Appendix B in the present report compares in a similar fashion the baseline characteristics and JTPA program experiences of our 18-month study sample and a nationally representative sample of JTPA participants.

[25] Appendix D describes the methodology used to obtain and test the site-specific impact estimates.

[26] Only 15 entries appear in the youth column of Exhibit 7.9 because one site did not include youths in its study sample.

[27] Sample sizes are not shown in the exhibits to protect the identity of the sites. For the 16 SDAs in the adult study and the 15 SDAs in the youth study, sample sizes average 405 for adult women, 276 for adult men, 153 for female youths, and 117 for male youths.

Exhibit 7.9 *Impacts on Total 18-Month Earnings: Adult and Out-of-School Youth JTPA Assignees, by Study Site*

Rank of site, by size of impact on adults or youths	*Impact per assignee, in $*	
	Adults (1)	*Youths* (2)
1	$ 1,851***	$ 2,566*
2	1,332	339
3	1,076	306
4	1,066	15
5	1,042**	-34
6	657	-34
7	620	-239
8	616	-315
9	507	-653
10	444	-707
11	414	-1,093
12	394	-1,268
13	55	-1,372
14	-323	-1,483*
15	-585	-3,591**
16	-745	—
F-test, difference among sites	n.s.	n.s.

Sources: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state UI agencies.

Notes: The adult and youth listings in each row do not necessarily refer to the same site. The study was limited to 15 sites for youths. For the estimation procedure, see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the F-test for the difference in impacts among the study sites is not statistically significant.

data. As shown in the last row of the exhibit, statistical tests for differences in impact across sites indicate that the site-specific estimates are not significantly different from one another for either the adult or the youth sample.[28] Thus, on the basis of these data, we cannot rule out the possibility that the impact of the program was the same in all sites and that the estimates differ purely by chance.

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[28] The tests fail to reject the joint hypothesis that impact was the same in all 15 or 16 sites considered. These tests are logically equivalent to separate tests that individual sites differ from the average of all other sites. These tests, like all others in the report, understate somewhat our uncertainty about program effects in relation to the national JTPA program, since they are not based on a probability sample of the nation. This limitation is especially important in this section when considering patterns of impact across sites, since it was in choosing 16 specific sites for the study on a non-statistical basis that national representativeness was lost. (As explained earlier, it was not possible to select sites—which had to voluntarily agree to the use of random assignment—on a strict statistical basis, despite major attempts to do so.) As statements about these particular sites, however, the significance levels shown here (and elsewhere in the report) accurately portray our confidence in the findings.

Nor can we be sure that program effects differed from zero in most of the individual sites examined. Exhibit 7.9 shows that 2 of the 16 site-specific impact estimates for adults are statistically significant, as are 3 of the 15 site-specific impact estimates for youths. While not large, this total—5 significant impact estimates among 31 tests—exceeds the number expected by chance alone—3—using tests that produce significant findings 1 time in 10 when no impacts occur. Hence, we would expect that at least some of the significant results in the exhibit signal local JTPA programs with real earnings impacts. Most notable is the $2,566 significant increase in earnings for youths in one site, a finding which differs strikingly from those seen earlier for youths in general and those shown here for 14 other sites. Evidently, one site considerably enhanced youth earnings even as the sites as a group reduced or left unchanged those earnings for the first 18 months of the follow-up period.

Analyses identical to those in Exhibit 7.9 were used to calculate site-level impacts for adult women and adult men separately, and for female youths and male youths separately, as shown in Exhibit 7.10. Here again, estimated effects differ widely but insignificantly across sites, leaving it uncertain as to whether true impacts differed across sites for any target group. Positive estimated effects for adult women and men, and negative estimated effects for male youths, do appear to be widespread, however. Significant positive effects appear in selected sites for the two adult target groups and significant negative effects for male youths. Still, the total number of significant findings—5 in 62 tests—is less than the number expected by chance (6). Hence, we cannot be sure that real effects occurred in any particular site.

A final question to be addressed here is whether local programs that worked best to increase earnings in one target group did the same for the other target groups. While this seems intuitively plausible, the reverse pattern could also hold if SDAs tended to focus their efforts on one or two target groups to the exclusion of the others, or if policies and approaches that worked well for some target groups did not do so for others. To examine these possibilities, we rearranged the site-specific impact estimates from Exhibits 7.9 and 7.10 into the three graphs shown in Exhibit 7.11. The first of these graphs displays the all-adult and all-youth findings from Exhibit 7.9 in graphic form, starting at the top with the site with the largest estimated impact on adults and continuing down the page to the site with the smallest (i.e., most negative) estimated impact for adults.[29] The size of each impact estimate is indicated by the length of the horizontal line segments, with the adult impact estimate shaded white and the youth impact estimate shaded gray for each site. Positive impact estimates extend to the right of center (the $0 point) and negative impact estimates extend to the left.[30]

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[29] Only 15 sites appear in Exhibit 7.11 because one site did not include youths in its study sample.

[30] All three graphs in Exhibit 7.11 use the same left-to-right scaling. Hence, the length of any two line segments—the size of any two impact estimates—is comparable across graphs, rather than their rank order.

Exhibit 7.10 *Impacts on Total 18-Month Earnings: JTPA Assignees, by Target Group and Study Site*

Rank of site, by size of impact on the target group	*Adult women (1)*	*Adult men (2)*	*Female youths* [a] *(3)*	*Male youths* [a] *(4)*
1	$ 1,738	$ 3,050**	$ 1,473	$ 4,424
2	1,583	1,487	1,105	1,597
3	1,249*	1,420	963	506
4	1,162**	1,123	830	335
5	1,058	1,064	805	-149
6	931	984	292	-656
7	587	910	-115	-1,165
8	474	811	-368	-1,199
9	390	557	-432	-1,604
10	254	478	-475	-1,654
11	217	191	-632	-1,969
12	127	14	-811	-2,249
13	111	-423	-914	-2,374*
14	-352	-1,084	-1,447	-2,643
15	-363	-1,124	-2,496	-6,581**
16	-655	-1,718	—	--
F-test, difference among sites	n.s.	n.s.	n.s.	n.s.

Sources: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state UI agencies.

Notes: The listings for the target groups in each row do not necessarily refer to the same site. The study was limited to 15 sites for youths. For the estimation procedure, see Appendix D.

a. Out-of-school youths only.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test)
"n.s." means the F-test for the difference in impacts among the study sites is not statistically significant.

Visually, it appears that SDAs with relatively positive impacts on adults also tended to have relatively positive (i.e., less negative) impacts on youths. A more formal test of this hypothesis—that some sites did relatively well with both adults and youths while others did poorly in both instances—is provided by the correlation coefficients that appear just below the graph.[31] These coefficients show that, in both rank order and dollar magnitude, the adult and youth findings correlate positively across sites, with correlation coefficients of .40 and .53, respectively, on a scale of -1 (perfect inverse correlation) to 1 (perfect positive correlation). If this pattern is not due simply to chance variations in the data,

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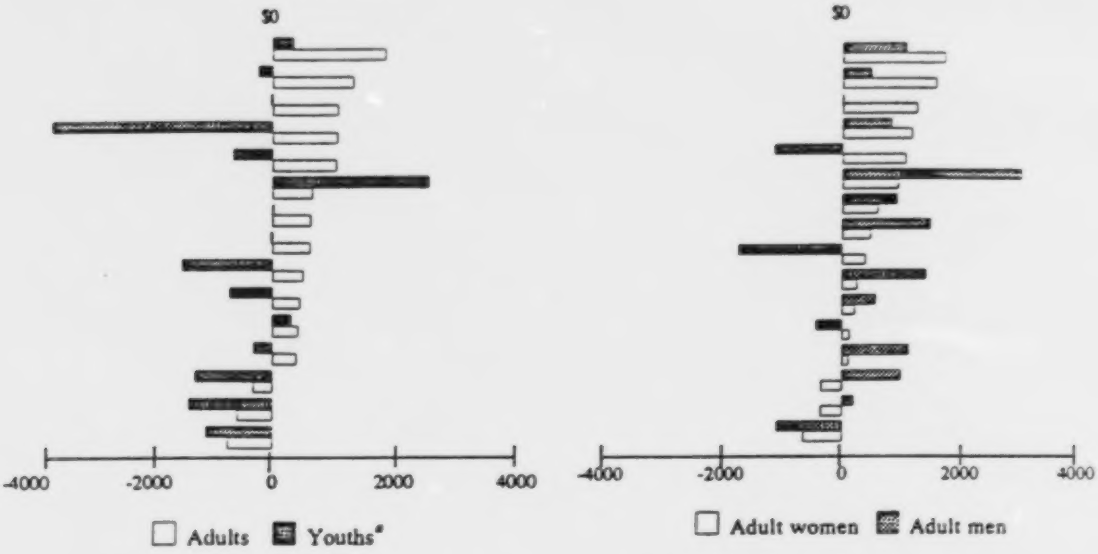
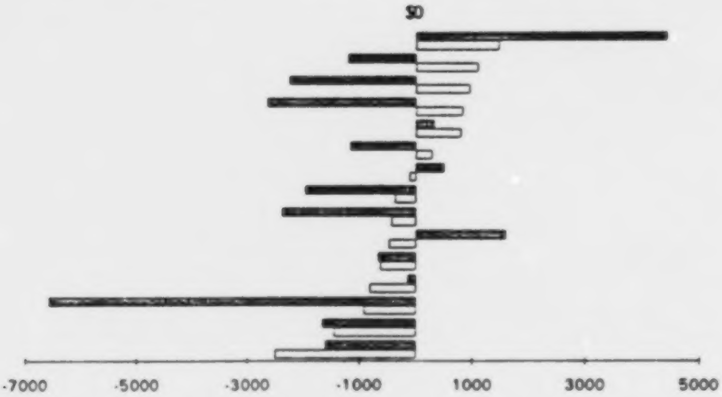
[31] The first of these coefficients, the rank-order correlation coefficient between the adult and youth impact estimates, shows the degree to which sites occupy similar positions in the two rankings. A value of 1 for this measure represents perfect correspondence between the two rankings, a value of 0 no pattern at all, and a value of -1 an exact reversal in the two patterns. The second measure, the dollar-value correlation coefficient, follows the same scale but is based on the dollar values of the impact estimates. See Appendix D for the formulas used in calculating each of these measures.

Exhibit 7.11 *Correlation of Impacts on Total 18-Month Earnings across Target Groups, by Study Site*



Rank order correlation coefficient = 0.40
Dollar value correlation coefficient = 0.53

Rank order correlation coefficient = 0.11
Dollar value correlation coefficient = 0.31



Rank order correlation coefficient = 0.19
Dollar value correlation coefficient = 0.62

Sources: Estimates of impacts per assignee, in dollars, based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state UI agencies.

Note: The pairs of horizontal bars are displayed in descending order by the size of the estimated impact for the target group listed first in the key to each graph.

a. Out-of-school youths only.

one can conclude that, among the 15 SDAs examined here, a local program's effectiveness in serving adults was positively related to its effectiveness in serving youths.

Similar graphs comparing adult women with adult men, and female youths with male youths, also appear in Exhibit 7.11.[32] For adults, there is only a weak positive relationship between a site's relative effectiveness serving women and its relative effectiveness serving men. Hence, knowing how well a site did in serving women tells us very little about how well it did in serving men. A somewhat stronger positive correlation exists between site-specific impact estimates for female youths and male youths. Hence, the SDAs that participated in the experiment achieved more consistent results across genders for youths than for adults.

To summarize these results, we cannot be sure that program effects differed across sites for any of the four target groups considered. Nor can we conclude that sites that produced the largest impacts for females did the same for males, although a consistent pattern of this sort does emerge when comparing all adults with all youths. In general, the overall lack of significant patterns in impact across sites may result from the small number of individuals analyzed at each site and the resulting uncertainty about true effects. Just as easily, it could mean that the 16 SDAs studied really did differ little in their impacts but appear to differ in our data due to chance variations alone.

The Influence of Selected Site and Baseline Characteristics on Program Impacts

A final component of the analysis seeks to explain patterns in estimated impacts by site in terms of local program characteristics, local labor market conditions, and participant baseline characteristics. The principal objective here is to understand how program operating decisions affected impact, in order to identify the local approaches that produced the best results. We have already seen (in the subgroup analysis) that participant baseline characteristics can also influence program impacts. Hence, we will need to control for baseline factors when measuring the influence of local program characteristics on program success. The influence of external factors such as local labor market conditions will also need to be controlled for in the analysis.[33]

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[32] Note that only 15 sites are included in the youth graph since one site did not include youths in its experiment.

[33] Unless we control for these factors, differences in baseline characteristics and local conditions could create the appearance that some SDAs operated their programs more effectively than others when, in fact, they merely faced a different set of initial conditions. Alternatively, differences in external conditions could mask important variations in operational effectiveness that would otherwise be apparent, as would happen, for example, if more effective SDAs faced atypically unfavorable labor market conditions.

The site-specific impact estimates shown earlier reflect the net effect of all of these factors taken together. Since those effects did not differ significantly across sites, we do not expect many of the site-level factors tested here to have statistically significant influences when examined separately. Still, it is only in looking for the local determinants of program impact that we have any chance of providing practical guidelines for improving local programs.

POSSIBLE DETERMINANTS OF IMPACT

In searching for the determinants of local JTPA impact, we begin by observing that measured program impact in any site depends on:

- Decisions made by the SDA in serving clients during the study period;
- The conditions faced by those clients when they entered the labor force, including characteristics of the local labor market and of the non-JTPA employment and training services available in the community (which should have influenced primarily earnings levels in the control group against which impacts are measured);
- The types of clients accepted for JTPA services; and
- The proportion of the client group studied—the assignee sample—that actually received JTPA services.

The potential for each of these factors to influence impact is fairly obvious. Some individuals may be better able to benefit from JTPA services than others, depending on their background characteristics, family situation, and prior educational and employment experiences. Similarly, the strength of the local labor market and the availability of services outside of JTPA may condition how much the program can do to increase participant earnings relative to what they would have earned without JTPA. Also, the type, quality, and duration of services provided through JTPA—determined by SDA operating decisions—will almost certainly influence program impacts. Finally, the greater the share of assignees served in an SDA, the larger the program impacts should be on the average assignee.

To identify the determinants of program effectiveness at the local level, we estimate program impact as a function of these four sets of factors—participant baseline characteristics, local environment, program approach, and the assignee enrollment rate—using data on individual sample members. The specific variables used are described below, beginning with the program measures on which the analysis focuses. These variables were chosen from a longer list of candidate measures as the ones most likely to (1) produce important variations in impact and to (2) vary substantially across individuals, local program offices, and/or entire SDAs.

Three aspects of *local program operations* are included in the model:

- *The percentage of assignees in the site recommended for the three different service strategies*—classroom training, OJT/JSA, and other services. Two sets of variables are used to represent the service strategy mix in a site: indicators of whether an individual was recommended for classroom training, OJT/JSA, or other services, and indicators of the percentage of assignees recommended to each service strategy for the site as a whole. Both sets of variables fall under the control of local program operators in deciding what service strategy will work best for individual clients and in determining the overall service strategy orientation of their programs. This latter factor—overall program orientation—could influence impacts in ways not reflected by service strategy decisions for individual clients and is included in the model separately for that reason. For example, SDAs that focused more on other services and less on traditional classroom and on-the-job training may have provided more innovative and individualized services in all service strategy subgroups.

- *The percentage of enrollees in the site facing two or more barriers to employment at baseline.* After controlling for the baseline characteristics of individual assignees (see below), this measure reflects an SDA's willingness and ability to concentrate its services on the more disadvantaged clients among those in its assignee pool.

- *The percentage of training dollars spent under performance-based contracts* in JTPA program year 1988, the midpoint of the service period for enrollees in the 18-month study sample. Sites that reimburse outside organizations for providing training services only if certain pre-specified performance levels are attained (e.g., placement in employment of a certain proportion of trainees) may have very different impacts than those who pay regardless of the training outcome.

All of these variables are measured separately for adults and youths and—except for the percentage of performance-based contracts—for females and males within the adult and youth groups.

Two measures of the *local labor market* are included in the model:

- *Urban/rural location* (coded separately for each local office in the 16 SDAs, based on population density and the characteristics of the office setting).
- *Local unemployment rate* (a monthly average for the period July 1987 to June 1990).

We use several variables to control for differences in *participant baseline characteristics* across sites, each one measured at the individual level for each assignee and control group member in the sample:[34]

- *Age at baseline* (variables distinguishing ages 22-29, 30-44, 45-54, and 55 and over for adults, and ages 16-19 and 20-21 for youths);
- *Gender;*
- *Ethnicity* (variables distinguishing whites, blacks, Hispanics, and others for adults, and blacks, Hispanics, and others—including whites—for youths); and
- *Barriers to employment at baseline* (separate indicators for individuals who had neither a high school diploma nor a GED and—for adults—individuals who worked less than 13 weeks in the previous year and individuals receiving cash welfare at application to JTPA).

Finally, to adjust for differences in the extent of services received by the assignees in the analysis sample, we add a variable measuring:

- *The percentage of assignees in each site enrolled in JTPA* during the 18-month follow-up period.

LIMITATIONS OF THE ANALYSIS

Before turning to results, we should note three limitations that apply to any attempt to attribute cross-site variations in impact to specific causal factors. Each of these limitations stems from a different source which, while independent in origin, have a cumulative effect on the overall reliability of the analysis:

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[34] Although it substantially influenced the size of program impacts for male youths, we do not include prior arrest record as a baseline variable in the model. Data on this characteristic are not available for adults.

- We can observe the influence of any particular local program characteristic only in combination with all of the other local program characteristics in the site. As a result, we must rely on a non-experimental model to separate out the role of each factor. Where those factors correlate with one another, or have non-additive effects, this separation will be only approximate. As a result, some portion of the influence of one factor may be mistakenly attributed to another factor.

- We have only 16 sites to work with, compared to dozens of local program characteristics that might conceivably influence program success. Because of this constraint, we can analyze only a handful of the candidate measures of interest, potentially omitting some that may strongly influence program impact.[35] Not only does this create the potential of overlooking an important program characteristic, it may also lead us to falsely attribute the influence of an omitted factor to included variables that happen to correlate with it.

- Finally, even if we manage to include all important program measures in the model and avoid confounding one with another, our ability to confidently quantify the influence of any one factor is constrained by the small samples sizes available in each site. To trace cross-site variations in impact to specific causal factors, we must first be able to measure those variations with confidence. We have seen already how imprecise the site-specific impact estimates are because of their small sample sizes.

Despite these limitations, the model used here provides the best information available on the sources of variation in local program effectiveness. Its strengths are that, in contrast to many previous studies of this sort, it:

- Uses a formal standard of evidence (statistical significance) to determine which factors are causal rather than coincidental;

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[35] Considering more variables by estimating the model on one set of candidate measures and then another would give misleading results. With repeated attempts, one or more versions of the model would almost certainly give an apparently complete and compelling explanation of the patterns in the data. But because it emerged from a "trial and error" process, what it would really represent is the "trial" that sooner or later was bound to fit the data well by chance alone once enough variations were tried. There is no reason to believe that a model selected on this basis correctly reflects the real sources of impact variation or has any generality for SDAs outside of the sample from which it was derived.

- Considers several factors at once in order to control for local labor market and participant characteristics when measuring the influence of local program characteristics; and

- Chooses in advance the factors to be considered, rather than letting potentially spurious patterns in the data dictate which factors are considered or advanced as causal.

FINDINGS

The findings from this model appear in Exhibit 7.12 for adults and youths separately.[36] Entries in the exhibit indicate which of the above variables are estimated to have had a statistically significant influence on the size of program impact in the study sites, holding constant the other variables in the model. Significant influences are marked by asterisks and discussed in detail below. Influences that are not statistically significant are indicated by "n.s." in the exhibit.

For adults, none of the program operating characteristics considered here significantly influenced program impacts. Moreover, the one significant influence shown—smaller impacts in sites that served a greater share of cash welfare recipients—emerged from a set of 15 tests and may reflect nothing more than chance variations in the data. Separate analyses of adult women and adult men (not shown) produced similar results.[37]

The results for youth are more interesting. Here, the operating characteristics of local JTPA programs as a group—and the mix of service strategies recommended in particular—had a significant influence on the size of program impacts.[38] Separate analyses (not shown) indicate that this pattern is confined to the male youth target group, since none of the variables in the model significantly influenced impacts on young women.

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[36] As before, the youth analysis is confined to the 15 SDAs that included youth in their experiments.

[37] The cash welfare variable was again statistically significant at the .10 level and negatively influenced impacts on adult women. The only other variable to significantly influence impacts for either adult women or adult men was the local unemployment rate, which had a significant (at the .10 level) negative effect on the size of program impacts on women.

[38] The significance of these influences was determined by first testing and rejecting the joint hypothesis that impact does not depend on any of the program operating characteristics included in the model and then by testing and rejecting the narrower joint hypothesis that impact does not depend on what service strategies are recommended for individual assignees or on the mix of service strategies employed by a site overall.

For young men, however, the mix of service strategies does correlate quite convincingly with the size of program impact, all other things equal.[39]

The nature of this correlation is quite complex, however. An examination of the individual regression coefficients (not shown) suggests that, among the 15 SDAs examined, those SDAs that focused their programs most squarely on the other services service strategy achieved the best (i.e., the least negative) overall impacts. Once the general orientation of a site is taken into account, however, recommendation to the other services strategy is *less* likely to produce favorable results for an *individual* assignee than is recommendation to classroom training or OJT/JSA.

Interpreting this pattern is not straight-forward. It may reflect in part the distribution of male youths with prior arrest records (whose estimated effects were much more negative than those of other male youths) across sites and service strategies. We plan to add the arrest variable to the model to sort out these relationships as part of the forthcoming 30-month follow-up report.

Exhibit 7.12 identifies one other source of impact variation for youths: residence in an urban versus rural location. Program impacts on 18-month earnings are estimated to have been $1,207 more negative for those youths who applied to JTPA at an urban office, all other things equal (not shown). This pattern is particularly striking among male youths, where there was an estimated urban/rural difference in impact of -$2,600. This may in part reflect the distribution of male youths with prior arrest records between urban and rural sites.

With these exceptions, none of the sources of impact variation across sites could be identified at conventional levels of statistical confidence. This is not surprising given the earlier conclusion that large measured variations in impact across sites may reflect nothing more than chance variations in the data—a consideration that should further caution against making too much of the statistically significant findings which were discovered. The major message of the site-level analysis, then, is that the available information cannot reliably distinguish between random "noise" in the data and true patterns of program impact across sites. If real impacts varied across the study sites and/or specific factors systematically influenced those impacts at the local level, the data provided by this study generally are not capable of measuring those variations with confidence. This, too, is unsurprising given that the study was not designed with this purpose in mind.

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[39] Joint tests of all program operating characteristics taken together and of the service strategy variables alone give statistically significant results at the .05 and .01 levels, respectively, for male youths

Exhibit 7.12 Influence of Selected Site and Baseline Characteristics, and Sets of Characteristics, on the Size of the Impact on Total 18-Month Earnings: Adult and Out-of-School Youth JTPA Assignees

	Impact per assignee, in $	
Set/Characteristic	*Adults (1)*	*Youths (2)*
Operating characteristics of the program (6 variables)	n.s.	**
Service strategies recommended (4 variables)	n.s.	**
% of enrollees facing two or more barriers to employment	n.s.	n.s.
% of training dollars spent under performance-based contracts	n.s.	n.s.
Characteristics of the local environment (2 variables)	n.s.	n.s.
Urban/rural location	n.s.	*
Local unemployment rate	n.s.	n.s.
Baseline characteristics of assignees (10 variables for adults, 5 for youths)	n.s.	n.s.
Gender	n.s.	n.s.
Ethnicity (3 variables for adults, 2 for youths)	n.s.	n.s.
Barriers to employment (3 variables for adults)	n.s.	n.s.
Receiving cash welfare [a]	*	--[b]
No high school diploma or GED certificate	n.s.	n.s.
Worked less than 13 weeks in past 12 months	n.s.	--[b]
Age (3 variables for adults)	n.s.	n.s.
Enrollment rate of assignees at the site	n.s.	n.s.
Sample size	*10,893*	*4,048*

Sources: Estimates based on First Follow-up Survey responses. The estimates for adult women are also based on earnings data from state UI agencies.

Notes: The influence is measured by the regression of individual sample members' 18-month earnings on site and assignees' baseline characteristics, an indicator of membership in the treatment group, and the interaction of the treatment group indicator and the site and baseline characteristics listed. Entries indicate the statistical significance of the interaction terms. For the estimation procedure, see Appendix D.

[a] AFDC, General Assistance, or other welfare except food stamps.

[b] Not tested in the regression.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the influence of the characteristic on the impact is not statistically significant.

A Summary of Cross-Target Group and Cross-Site Comparisons

The results presented in this chapter focus on the nature and origins of variation in impacts across target groups and across sites. By cataloging these variations in relation to other measured differences between target groups and sites, we take the first tentative step toward understanding the nature of the impact findings presented elsewhere in the report.

TARGET GROUPS

Taken as a whole, the effects of JTPA on the four target groups differ substantially. The major contrast concerns differences between adults and youths, although systematic male/female differences are also evident, especially for youth. In brief, the four target groups can be summarized as follows:

- *Adult women* most clearly benefited from JTPA participation. This benefit came late in the follow-up period if recommended for classroom training, throughout the period if recommended for OJT/JSA, and early in the period if recommended for other services. Usually, these earnings gains were attained by working more hours rather than earning more per hour, although the reverse was true of the classroom training subgroup. Additional services received through access to JTPA concentrated most heavily on classroom training in occupational skills.

- The evidence of positive impacts on *adult men* generally parallels that for adult women but is not quite as strong. Men differed from women in several important respects, however. They earned substantially more absent JTPA and were more likely to obtain on-the-job training because of JTPA. And unlike adult women, classroom training did not increase adult men's earnings by allowing them to earn more per hour.

- Unlike the two adult groups, *female youths* seem not to have benefited from JTPA participation in general. They earned less absent the program than any other target group and tended to receive additional hours of classroom training (both in occupational skills and basic education) when allowed access to JTPA. The program noticeably affected their earnings in two instances: early losses in earnings within the classroom training subgroup (where hours worked declined substantially) and substantial but statistically insignificant earnings gains in the OJT/JSA subgroup (where earnings per hour rose substantially).

- *Male youths* were the one target group which experienced a negative impact from the participation in JTPA, as measured by the follow-up survey data, especially those with records of prior arrests.[40] Large, sustained losses in earnings typified the male youth experience with the OJT/JSA service strategy and the other services strategy (which provided primarily miscellaneous services and basic education). These losses resulted primarily from substantial decreases in hours worked. Male youths were more often recommended for the other services subgroup than female youths but otherwise received very similar services.

SITES

A comparison of estimated impacts across sites revealed little about the sources and nature of JTPA impacts, in part because of the small samples available in each site. In most sites, access to JTPA is estimated to have increased the earnings of adult assignees and reduced the earnings of youths, although these patterns are not statistically significant in most sites. There is also some suggestion in the data that sites which achieved relatively more favorable outcomes for adults also tended to do so for youths.

The degree of variation in impact across sites is uncertain. For no target group was this variation statistically significant, and only rarely could it be traced to specific site-level characteristics. None of the factors hypothesized to influence earnings gains at the local level—program operating features (e.g., the mix of service strategies employed), local labor market conditions (e.g., unemployment rate), and participant characteristics (e.g., barriers to employment)—were found to have done so for adult women, adult men, or female youths. The two local factors found to significantly influence program impacts for male youths—the mix of service strategies employed by the local SDA, and urban/rural setting—may no longer do so once the distribution of male youths with prior arrest records is equalized across sites and service strategies in a later analysis.

SUMMARY

The adult/youth contrast which predominates these findings may change once longer-term follow-up data become available, or once program costs are considered in the final benefit-cost analysis. Either way, future research should seek a clearer understanding of why these short-run differences in impact arose. Three preliminary pieces of evidence are available already:

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[40] As noted in Chapter 6, a secondary data source (unemployment insurance wage records) does not show large negative effects on male youths. The contrast in these two findings will be investigated further in the final report for the study.

- First, it is possible that the adult/youth differences in impact are the result of age alone. As discussed in Chapter 6, impacts on youths do not differ significantly from what is expected for young assignees given the downward trend in impacts that emerges when examining successively younger subgroups of adults. For men, however, estimated impacts for 16- to 21-year-olds lie considerably below this trend line—perhaps due to large negative effects on male youths with prior arrest records—though the departure from the trend is not statistically significant. Hence, we cannot be sure that the adult and youth programs differed systematically in their effectiveness for either gender.

- Second, to the extent that program differences do account for observed differences in impact between adults and youths, they do so without creating large measured differences in service receipt. In terms of recommended services, the most striking differences distinguish women from men, not adults from youth. Moreover, differences in impact between adults and youth are just as evident when sample members are grouped by service strategy as for target groups as a whole. In the one service strategy subgroup where the impact contrast is sharpest—OJT/JSA—there is essentially no difference by age in the additional hours and types of services obtained through access to JTPA. Differences for the other two service strategies are more apparent, but still modest.

- Third, it is possible that earnings reductions for male youths resulted in part from the mix of service strategies employed. Service strategies relate to program impacts in a complex way, however, and may be confounded with differences in impacts for youths with and without records of prior arrest.

Finally, it is important to note that, even if the adult and youth programs differed in important and perhaps unmeasured ways, we should not conclude that JTPA is necessarily as capable of helping youths as it is adults. As noted earlier, youths have somewhat different baseline characteristics and earnings potential absent the program, factors that may or may not limit their ability to benefit from a given JTPA treatment. So we cannot be sure that equalizing treatment would necessarily equalize effects. Thus, while the program has clearly found a way to improve the short-run labor market outcomes of adults, we do not yet know how it might move closer to that objective for female youths and—in the case where change is most clearly needed—male youths.

Appendix A

A Statistical Comparison of Treatment-Control Group Differences in Baseline Characteristics

THIS appendix describes how we used a multivariate discriminant analysis to determine the comparability of the treatment group and the control group in the 18-month study sample, using the data on baseline characteristics that were presented in Chapter 3 (exhibits 3.13 and 3.14). Three potential problems must be addressed in conducting such an analysis:

- A *multiple comparisons problem* arises when one tests many hypotheses. Doing so is likely to produce by chance significant test statistics for some of the hypotheses, even when the populations being compared are identical. For example, when testing differences for statistical significance at the .05 level, 1 out of 20 independent tests will be significant when in fact no real differences exist.

- *Interdependencies* (correlations) among the different test statistics occur because characteristics are usually distributed across individuals in patterns, not randomly. For example, income and education are often correlated. Hence, a test of differences in income between a treatment group and a control group would not be independent of a test that compared the educational levels of the two groups.

- *Missing data* are virtually inevitable in any large-scale empirical study. Here the problem is that different data usually are missing for different sample members. In the model described below, we avoided the problems of multiple comparisons and interdependencies among the separate comparisons by "pooling" the analyses of separate baseline characteristics. In addition, to avoid deleting sample members from the analysis when data were missing for some, but not all, of the baseline characteristics measured, we specified the statistical model used to compare treatment group and control group members to account for patterns of missing data.

The Basic Statistical Model

A standard form of the multivariate discriminant model is based on the assumption that the characteristics in two populations (in this case, the populations from which the treatment and control groups were drawn) have a joint normal distribution in both populations, with a common covariance matrix but different mean values. Under this assumption we can generate a discriminant function for the probability that an observation (sample member) with given characteristics comes from one of the two populations. Furthermore, as developed by Fisher (1938), we can test the hypothesis that the measured characteristics do not discriminate between the two populations (in other words, that the mean values for the two populations are the same) by using the usual composite test for a set of estimated coefficients from an ordinary least squares regression of the form:

$$TREATMENT_i = a + \sum_k b_k CHAR_{ki} + e_i, \qquad \text{(A1)}$$

where

$TREATMENT_i$ = 1 for treatment group members, and 0 for control group members;

$CHAR_{ki}$ = the value of baseline characteristic *k;*

a = an intercept; and

e_i = a random error.

To summarize the overall difference between the sets of baseline characteristics of the treatment group and the control group, we used the R^2 produced from estimating a model of this type by ordinary least squares. To determine the statistical significance of this overall difference, we used a standard F-test for the hypothesis that the coefficients of the characteristics in Equation A1 were all zero. This test is mathematically equivalent to the test for a multivariate discriminant model that indicates the statistical significance of the difference between two multivariate normal distributions.[1]

The foregoing procedure enabled us to account for interdependencies among the treatment-control group comparisons for different baseline characteristics and to solve the multiple comparisons problem that arises when making comparisons of many different characteristics by testing a single composite hypothesis.

Interdependencies among the different baseline characteristics were accounted for by including them together on the right-hand side of a multivariate model and conducting a joint test of their overall significance. In such a model correlations among right-hand-side variables are reflected in the variance-covariance matrix that is the basis for parameter estimates and test statistics. Thus, for example, the R^2 measure we used to summarize treatment-control group differences evaluates the set of baseline characteristics as a group and nets out overlapping effects of separate variables.

The single joint test we used to determine the statistical significance of treatment-control group differences for each target group in effect summarized the results of many comparisons into a single one. This reduced the chance of observing random, but seemingly significant, differences, as tends to happen when employing a larger number of tests.

Extension of the Basic Model

An extension of the model was necessary to account for partial missing data. Specifically, we added a series of dummy variables on the right-hand side to indicate the presence or absence of data for each baseline characteristic. These dummy variables were defined as follows:

$DATA_{ki}$ = 1 if data on baseline characteristic k were available for sample member i, and 0 if not.

Because in some cases two or more baseline characteristics were measured using responses from related questions on the Background Information Form, the missing data patterns for these characteristics were the same. In these cases we therefore used a single data availability indicator for each set of baseline characteristics.

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[1] See Fisher (1938) or, for a simpler discussion, Haggstrom (1983).

A final, related extension of the basic model was to set the value of each baseline characteristic, $CHAR_{ki}$, equal to zero for those sample members who were missing data on this characteristic. This redefined characteristic variable, $CHAR'_{ki}$, is an interaction between the original characteristic variable and its corresponding data availability indicator.[2] The final multivariate discriminant model was therefore specified as follows:

$$TREATMENT_i = a + \sum_k b_k CHAR'_{ki} + \sum_k d_k DATA_{ki} + e_i. \qquad \text{(A2)}$$

We used ordinary least squares to estimate this model from data on all members of each target group.

The Analysis and Findings

To pool the comparison of all the baseline characteristics into one joint test per target group, we computed the incremental R^2 for the set of characteristics variables, $CHAR'_{ki}$, for that target group. The incremental R^2 is the difference between the R^2 for the full model for the group and the R^2 for a model that omitted the $CHAR'_{ki}$.[3]

The incremental R^2 for the baseline characteristics is a direct measure of the squared correlation that will exist between the baseline characteristics and treatment group or control group status when these variables are used in the right-hand side of multiple regression models for estimating program impacts. This correlation will reduce the

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[2] To see how missing data indicators and their corresponding baseline characteristics can be interpreted in the model, consider an example with one characteristic. Suppose that:

$RACE'_i$ = 1 for whites, 0 for nonwhites, and 0 for sample members with missing data; and

$DATA_i$ = 1 for sample members with data on this characteristic, and 0 for those with missing data.

The treatment-control group discriminant model for only this factor would be:

$$TREATMENT_i = a + b_1 RACE'_i + c_1 DATA_i + e_i.$$

The coefficient c_1 in this model measures the difference in missing-data rates for treatment group and control group members. The coefficient b_1 measures the treatment-control group difference in the average value of $RACE'_i$ for sample members with data on this variable.

[3] This parameter represents the overall difference in baseline characteristics between the treatment and control groups, after we controlled for the rates of missing data.

precision of program impact estimates. Specifically, minimum detectable effects[4] will be inflated by a factor (IF_{mde}), defined as:

$$IF_{mde} = \frac{1}{\sqrt{(1-R^2)}} , \tag{A3}$$

where R^2 is the incremental R^2 for the baseline characteristics and, hence, reflects the correlation between these characteristics and a dummy variable that distinguishes treatment group members from control group members.[5] For example, if the R^2 were 0.01, the minimum detectable effect would be 1.005 times what it would have been if the R^2 had been zero. This would increase a $500 minimum detectable effect to $502.50.

Exhibit A.1 presents the incremental R^2 for each target group in the 18-month study sample, while Exhibit A.2 lists the variables used in the comparison. As shown in the first exhibit, the incremental R^2s range from 0.0032 to 0.0128, which implies the minimum detectable effects will be inflated by factors ranging from 1.002 to 1.006. In other words, the measured differences between the treatment group and control group within each target group will reduce the statistical precision of the program impact estimates for each target group by a negligible amount.

A second key finding shown in Exhibit A.1 involves the statistical significance of the overall differences in baseline characteristics between the treatment and control groups. We addressed this issue by computing the F-statistic for each incremental R^2 and determining the probability of an F-value being equal to or greater than the observed value under the null hypothesis of no difference between the baseline characteristics of the treatment and control groups. This probability, or *p-value*, is the lowest test level for which the observed differences would be statistically significant.

The second row of Exhibit A.1 displays this *p-value* for each target group. In the case of adult women, for example, the difference in baseline characteristics for the treatment and control groups is statistically significant at only the .95 level. This implies a 95 percent chance of observing treatment and control group characteristics that differ by as much as or more than those for the 18-month study sample of adult women, when there is no difference, on average, in the population from which they were sampled. The corresponding significance levels for adult men, female youths, and male youths were 0.29, 0.40, and 0.94, respectively. Hence, no treatment and control group differences were significant at the conventional 0.10 or 0.05 levels.

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[4] In simplest terms, a *minimum detectable effect* is the smallest real program effect that has a good chance of being detected—that is, yielding a finding of a statistically significant impact—using the data available.

[5] This statement reflects the fact that a standard error of estimate for a regression coefficient is inversely proportional to the *independent* standard error of the right-hand-side variable to which it refers, where the independent standard error is based on the variation in the variable that is not correlated with any other covariates.

In summary, then, the findings of this statistical analysis indicates virtually no difference in measured baseline characteristics between the treatment and control groups within each target group. Those differences that were observed are neither substantial (as measured by the incremental R^2) nor statistically significant (as measured by the *p-value* for the incremental R^2). Our findings are therefore in accord with expectations for a properly designed and executed random assignment process.

Exhibit A.1 *Results of the Multivariate Discriminant Analysis of Baseline Characteristics of the Treatment Group and Control Group: The 18-Month Study Sample, by Target Group*

	Adult women (1)	*Adult men (2)*	*Female youths[a] (3)*	*Male youths[a] (4)*
Incremental R^2	0.0032	0.0065	0.0128	0.0100
p-value for the incremental R^2	0.95	0.29	0.40	0.94
Sample size	*6,607*	*5,626*	*2,649*	*2,144*

a. Out-of-school youths only.

Exhibit A.2 *Baseline Characteristics Used as Regressors in the Multivariate Analysis of Treatment-Control Group Differences*

Characteristic
Ethnicity
White, non-Hispanic
Black, non-Hispanic
Hispanic
Asian or Pacific Islander
(American Indian or Alaskan Native)
Education and training history
High school diploma or GED certificate
(No high school diploma or GED certificate)
Previously received occupational training
(Received no occupational training previously)
Work history
Ever employed
(Never employed)
Employed upon application
(Not employed upon application)
Mean individual earnings in past 12 months
Weeks worked in past 12 months
Hourly earnings in most recent job
Hours worked in most recent job
Public assistance status
Receiving AFDC
(Not receiving AFDC)
Receiving food stamps
(Not receiving food stamps)
Receiving other cash assistance[a]
(Not receiving other cash assistance)[a]
Receiving housing assistance
(Not receiving housing assistance)

(Continued)

Exhibit A.2 *Baseline Characteristics Used as Regressors in the Multivariate Analysis of Treatment-Control Group Differences (continued)*

Characteristic
AFDC history
Ever AFDC case head
(Never AFDC case head)
AFDC case head 5 years or more
AFDC case head 2 or more, but less than 5, years
(AFDC case head less than 2 years)
JTPA required for welfare, food stamps, or WIN program
Yes
(No)
Household composition
Spouse present
(No spouse present)
Own child, any age, present
(No child present)
Number of children under age 6 present
Family income in past 12 months
< $3,000
$3,000 - $6,000
$6,001 - $9,000
$9,001 - $12,000
$12,001 - $15,000
(> $15,000)
Living in public housing
Yes
(No)
Age at random assignment

Notes: Characteristics in parentheses were left out of the regression equations to avoid overdetermination of the multivariate model. In addition to the characteristics mentioned here, a constant and 19 data-availability indicators were used in the equations.

a. General Assistance or other welfare except AFDC, food stamps, and housing assistance.

b. WIN is the federal Work Incentive program.

Appendix B

A Comparison of JTPA Enrollees in the 18-Month Study Sample and the Title II-A Participant Population Nationwide

THIS appendix compares JTPA Title II-A enrollees in the 18-month study sample with two national comparison samples of adults and out-of-school youths who were enrolled in JTPA Title II-A during the sample intake period for the National JTPA Study (November 1, 1987 to September 30, 1989). The first section describes how the national comparison samples were constructed. The second section compares the baseline characteristics of the JTPA enrollees in the 18-month study sample with those of the national comparison samples, while the third section compares the in-program experiences—duration of enrollments, program services received and JTPA performance indicators—for the samples. A summary of these comparisons forms the final section of this appendix.

Construction of the National Comparison Samples

Two related comparison samples were constructed to represent the population of adults and out-of-school youths who entered JTPA Title II-A programs nationally during the sample intake period for this study: one for JTPA enrollees, and one for JTPA terminees. These national comparison samples were constructed from information in the Job Training Quarterly Survey (JTQS), an ongoing data collection effort conducted by the U.S. Bureau of the Census (under contract to the U.S. Department of Labor) and reported by Westat, Inc.[1]

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[1] See Westat Inc. (1988).

The JTQS obtains data quarterly from the administrative records of a probability sample comprising 142 of the 649 JTPA service delivery areas (SDAs) nationally. Two JTQS samples are drawn each quarter: a sample of enrollees from the 142 sites, which is drawn from SDA records on persons who became enrolled in JTPA during the quarter (regardless of when they were terminated from the program); and a sample of terminees, which is drawn from SDA records on persons who were terminated from JTPA during the quarter (regardless of when they were enrolled).

To construct our *national comparison sample of JTPA enrollees*, we obtained all the JTQS enrollee sample data on adults and out-of-school youths who became enrolled in JTPA Title II-A between November 1, 1987 and September 30, 1989.[2] JTQS sample members who were in-school youths or who became enrolled in JTPA before November 1, 1987 or after September 30, 1989 were thus excluded from the sample. In the discussion that follows we compare the baseline characteristics of the 12,289 members of this national comparison sample of JTPA enrollees and the baseline characteristics of the JTPA enrollees in the 18-month study sample.

Further comparisons with regard to in-program experiences are not possible, however, because information on the duration of enrollments, program services received, and program performance indicators is based on the *termination* date and status of JTPA participants, which are not available for many members of the JTQS enrollee sample who had not yet completed their JTPA enrollment when their data were collected.

We therefore constructed a *national comparison sample of JTPA terminees*, obtaining all JTQS terminee sample data on those terminees who became enrolled in Title II-A during the sample intake period for this study (again, from November 1, 1987 to September 30, 1989).[3] Excluded from this sample were in-school youths and persons who became enrolled in JTPA before November 1, 1987 or after September 30, 1989. Thus, although this sample is based on terminee records, it reflects the national population of adults and out-of-school youths who were *enrolled* in JTPA Title II-A during the sample intake period for this study.[4]

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[2] This information is in the JTQS Public Use File produced by Westat, Inc., for the period that began in the second quarter of program year 1987 (October to December 1987) and ended in the first quarter of program year 1989 (July to September 1989).

[3] This information is in the JTQS Public Use File produced by Westat, Inc., for the period that began in the second quarter of program year 1987 (October to December 1987) and ended in the fourth quarter of program year 1989 (April to June 1990).

[4] In other words, by choosing the national comparison sample of JTPA terminees from within a representative national sample of terminees for a period that spanned the study sample intake period and extended nine months beyond it, we obtained a nationally representative sample of adults and out-of-school youths who became enrolled in JTPA during our intake period for whom complete data on their enrollment period, services received, and program performance indicators were available.

Later in this appendix we compare the subsequent in-program experiences and performance indicators of the 13,434 members of this national comparison sample of JTPA terminees with those of JTPA enrollees in the 18-month study sample. In the next section we also present comparisons using the terminees' baseline characteristics; but because the terminee sample may be slightly less complete than the enrollee sample,[5] primary emphasis in the comparisons of baseline characteristics should be placed on the JTQS enrollee sample. Nonetheless, there was not much difference in baseline characteristics between the two national comparison samples.

A Comparison of Baseline Characteristics

For the purposes of this comparison, we constructed measures of baseline characteristics that were defined in the same way as the baseline characteristics that were measured for the 18-month study sample using Background Information Form (BIF) responses.[6] To minimize the potential for noncomparability between the BIF data and the JTQS data, we excluded any measured baseline characteristic with missing data for more than 20 percent of either sample.

Exhibits B.1 through B.4 display the findings for the 16 baseline characteristics (15 for youths) for which comparable BIF and JTQS data were available. Each exhibit presents the findings for a separate target group. The baseline characteristics of enrollees in the 18-month study sample are expressed as simple means and percentages, while those of the two national comparison samples are expressed as weighted means and weighted percentages, based on the JTQS sampling weights in the Public Use Files.[7]

Overall, the results in the four exhibits indicate that the measured baseline characteristics of enrollees in the 18-month study sample were quite similar to those of adults and out-of-school youths who became enrolled in JTPA Title II-A programs nationwide during the intake period for the National JTPA Study. For none of the baseline characteristics measured was there a difference between enrollees in the 18-month study sample and either of the two national comparison samples that was consistently large across all four target groups. In addition, there is no clear pattern to the few noticeable differences that appear within each target group.

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[5] U.S. Department of Labor (1987, 7n.).

[6] The only difference between the baseline characteristic definitions employed in the chapters of this report and those used here is that, unlike in the chapters, the definition of "any public assistance" used here does not include unemployment insurance—receipt of which is not measured in the JTQS files.

[7] Westat, Inc. (1989) describes the JTQS sampling design and the weights that reflect this design.

Because of the complex sampling design for the JTQS, it is not possible to calculate whether any baseline characteristics of the national comparison samples were statistically significantly different from those of enrollees in the 18-month study sample.[8] Hence, many if not all of the differences reported in the exhibits may reflect random sampling error rather than systematic differences among the samples.

Turning to the specific results, we find that the 18-month study sample closely resembled the national comparison samples in terms of age. Most adults were about evenly split between the ages of 22 to 29 and 30 to 44. The average age of adult enrollees was about 33 in the 18-month study sample and about 34 in the national comparison samples. This slight difference is due mainly to the fact that the adults in the 18-month study sample were less likely to be over 54 years old, because six of the study sites excluded older applicants from participation in the experiment. Older workers were not excluded from the JTQS and thus appear with greater frequency in the national comparison samples. The youth samples were also very comparable in age, with enrollees having exactly the same mean age in the 18-month sample and the national comparison sample of JTPA enrollees.

The ethnic compositions of the samples were also quite similar. Whites made up around half of both samples; blacks, from a quarter to a third; and Hispanics, from 10 percent to 19 percent. The ethnic mix of adult men in the 18-month study sample was very close to that of the national comparison samples. Both adult women and female youths were, however, less apt to be black—and female and male youths more apt to be Hispanic—if they were in the 18-month study sample than if they were in the national comparison sample of enrollees.

The educational backgrounds of adult enrollees in the 18-month study sample were similar to those in the national comparison samples. Over two-thirds of the women and men in all three samples had a high school diploma or a GED certificate. But the youth enrollees in the 18-month study sample were less apt to have a high school credential than were their counterparts in the national comparison samples.

Specifically, 47.5 percent of the female youth enrollees in the 18-month study sample had a high school credential versus 59.1 percent and 55.7 percent of the female youths in the national comparison samples. Only 38.3 percent of the male youths in the 18-month study sample had a high school credential versus 44.7 percent and 45.5 percent of the male youths in the national comparison samples. Hence, it appears that youth enrollees in the 18-month study sample were more educationally disadvantaged than were their counterparts in the national comparison samples, although, again, it was not possible to determine whether this difference was statistically significant.

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[8] Westat, Inc. (1989) describes the likely effect of the JTQS sampling design on the statistical properties of the data for the sample.

The employment status of enrollees in the 18-month study sample was very similar to that of members of the national comparison samples. In all three samples only about 12 percent to 16 percent of the target group members were employed when they applied to JTPA.

The final panel in the exhibits indicates the receipt of public assistance by sample members. Across all four target groups the enrollees in the 18-month study sample were slightly less likely to be receiving any public assistance and AFDC in particular than were the members of the national comparison samples. In all three samples, however, the males (both adults and youths) were far less likely than the females to be receiving public assistance, especially AFDC. Thus, the patterns of receipt of public assistance across gender categories was the same for both samples. Furthermore, enrollees in the 18-month study sample were about as likely as, or more likely than, the national comparison samples to be receiving food stamps when they applied to JTPA. Hence, there was no consistently large difference in the *patterns* of receipt of public assistance.

In summary, then, it appears that the baseline characteristics of the enrollees in the 18-month study sample were generally similar to those of the national comparison samples. No characteristics differed appreciably for all four target groups and there is no clear pattern to the few noticeable differences that exist. The main exception is that the youth enrollees in the 18-month study sample were less likely to have a high school credential than were their counterparts in the national comparison samples.

A Comparison of In-Program Experiences

Exhibits B.5 through B.8 compare the duration of enrollments, the JTPA services received, and key performance indicators for the study sample enrollees and the national comparison sample of JTPA terminees. As noted earlier, this information is complete for only those persons with a complete JTPA enrollment record—that is, terminees; the exhibits therefore do not present information on the national comparison sample of enrollees. Complete enrollment records were available for the national comparison sample of JTQS terminees and for at least 18 months of follow-up for the enrollees in the 18-month study sample.[9]

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[9] In both cases there were some missing data for specific items, as would be expected.

The data for the comparison sample of terminees represent a single spell of enrollment in JTPA Title II-A per sample member.[10] SDA records on study sample enrollees indicate, however, that 96.5 percent of this group had one enrollment spell in a Title II-A program, 3.5 percent had two spells, and less than one tenth of a percent had three spells during their 18-month follow-up period. To measure the in-program experiences of the study in the same way as the experiences of the comparison sample of JTPA terminees were measured, we examined data on the *first* Title II-A enrollment spell only. Hence, for both samples exhibits B.5 through B.8 present means and percentages for a single spell of enrollment in Title II-A for each sample member. The data for the national comparison sample of JTPA terminees were weighted using their JTQS sampling weights.

The first panel in the exhibits presents the mean duration of enrollments for the two samples.[11] These results indicate that the enrollment periods for the two samples were quite similar, on average, for all the target groups except adult women. Adult women in the 18-month study sample had a somewhat shorter average enrollment period, 4.0 months, than that of their counterparts in the national comparison sample of JTPA terminees, 4.5 months.[12]

The second panel in the exhibits displays the percentage of sample members who received each of the six categories of specific JTPA services. It is in this regard that enrollees in the 18-month study sample differed the most from the national comparison sample of JTPA terminees. Specifically, for all four target groups:

- enrollees in the 18-month study sample were *more* likely to receive *classroom training in occupational skills* and *job search assistance* than were members of the national comparison sample of terminees; and
- enrollees in the study sample were *less* likely to receive *on-the-job training* and *miscellaneous services* than were members of the national comparison sample of terminees.

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[10] Hence, the national comparison sample of JTPA terminees is a nationally representative sample of "completed" JTPA enrollment spells that began during the sample intake period for the National JTPA Study.

[11] Termination dates were missing for 10.3 percent of the enrollees in the 18-month study sample (13.9 percent of the adult women, 6.9 percent of the adult men, 10.4 percent of the female youths, and 8.0 percent of the male youths). Because none of the recorded enrollment periods was longer than 12 months, the fact that termination dates were not recorded for these sample members during their 18-month follow-up period probably represents missing data rather than enrollment periods that were longer than 18 months.

[12] Mean enrollment periods for the 18-month study sample in exhibits B.5 through B.8 are longer than the corresponding *median* enrollment periods shown in Exhibit 3.20 in Chapter 3, as would be expected.

In addition, for all the target groups except adult women, enrollees in the study sample were *more* likely to receive *basic education* than were members of the comparison sample of terminees, although the differences for this fifth category of services were smaller than those for the other four.

It is unlikely that all of these differences in service receipt are due to differences in how the data for the two samples were collected or how specific JTPA services were defined. Measures of service receipt for both samples were constructed from SDA enrollment and tracking data on individual sample members. And the definitions of each specific program service from each data source were carefully compared.[13]

On the other hand, because it is not possible to determine whether the differences were statistically significant, it is difficult to know whether they represent the result of random error due to sampling or real, systematic differences between the study sample and its counterpart of adults and out-of-school youths in Title II-A programs nationally. Nevertheless, given the magnitude and consistency of the observed differences in the mix of services received by the two samples, the data suggest but cannot prove that these samples represent groups that received somewhat different services from JTPA.

Despite these potential differences, however, the second panel also suggests that the two samples were similar in the *patterns* of service receipt *across target groups*. Perhaps most striking in this regard is the much greater likelihood of receiving basic education among youths than among adults in the two samples. Second is the much greater likelihood of receiving classroom training in occupational skills among females (both adults and youths) than among males. And third is the greater likelihood of receiving on-the-job training among males than among females.

The last panel in exhibits B.5 through B.8 indicates very little difference in key indicators of program performance between the 18-month study sample and the national comparison sample.[14] First, for all the target groups except female youths the *entered employment rate* (the percentage of sample members who had found a job before terminating their enrollment in JTPA) was very similar for members of both samples; for female youths the entered employment rate was 51.1 percent for enrollees in the 18-month study sample and 57.1 percent for the national comparison sample of terminees. Second, for all except adult women the *average hourly wage* for sample members who were employed when they were terminated from Title II-A was very similar for the two samples. For adult women this "placement wage" was $5.06 for members of the study sample and $5.99 for members of the national comparison sample.

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[13] Published JTQS reports use an algorithm to classify each sample member according to the *primary* service (referred to as a "program activity") that he or she received, and thus include each sample member in one service category only. Here, to match the way that service receipt rates are measured throughout this report, we include each JTQS sample member in *every* service he or she received, and thus, each sample member can be represented in more than one service category. The findings in exhibits B.5 through B.8 are therefore not directly comparable to those in the published JTQS reports.

[14] Of the 6,559 enrollees in the 18 -month study sample who had a valid termination date, 24 were missing data on whether or not they were employed upon their termination from JTPA. Of the 4,200 enrollees in the 18-month study sample who had a valid termination data *and* who were employed upon termination, 420 were missing data on the wage rate for their job.

Hence, there is no evidence that the study sample examined in this report experienced a level of program performance that differed appreciably from that experienced by members of the national comparison sample of JTPA terminees.[15]

Summary

Although the 18-month study sample was not drawn from a national probability sample of SDAs, it resembled its counterpart of adults and out-of-school youths in the Title II-A program nationally. Nevertheless, it also differed from the two national comparison samples in several important ways.

In terms of baseline characteristics the 18-month study sample is quite similar to its national counterpart. The only appreciable difference is that out-of-school youth enrollees in the 18-month study sample were less likely than out-of-school youths in Title II-A programs nationally to have a high school diploma or GED certificate when they applied to the program.

In terms of their experiences in JTPA programs the enrollees in the 18-month study sample had about the same average duration of enrollments as their counterparts nationally, they were about equally likely to be employed upon their termination from the program, and they earned about the same average hourly wage if they were employed.

In terms of JTPA services received, however, the enrollees in the 18-month study sample were more likely to receive classroom training in occupational skills and job search assistance, and less likely to receive on-the-job training and miscellaneous services, than were members of the national comparison sample of JTPA terminees. Nevertheless, the patterns of differences in service mixes across target groups were quite similar for the two samples. In both the study sample and the national comparison sample of terminees, the youths were more likely than the adults to receive basic education, the females were more likely than the males to receive classroom training in occupational skills, and the males were more likely than the females to receive on-the-job training. In this regard the experiences of the 18-month study sample reflect decisions by local SDA staff, entry requirements and service availability of local programs, and personal preferences of the applicants themselves that appear to be similar to those which determined the mix of services received by Title II-A participants across the country.

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[15] A third key indicator used to measure JTPA performance for youths is the "positive termination rate," which is the percentage of all youth terminees who, before terminating their JTPA enrollment, had found a job, attained recognized employment competencies established by the Private Industry Council (PIC), completed elementary, secondary, or post-secondary school, enrolled in another training program or an apprenticeship, enlisted in the Armed Forces, or returned to school full-time. Because data on positive termination rates were missing for more than 20 percent of the youth enrollees in the 18-month study sample, we did not compare findings on this indicator.

But whatever the differences or similarities we may observe between the 18-month study sample and the two national comparison samples, there is no way to determine how the program impacts produced at the 16 study sites compare to those produced by JTPA Title II-A programs nationally, because there is no valid measure of average program impacts nationally.

Hence, we make no claims about the national representativeness of the impact estimates presented in this or any other report for the National JTPA Study. Instead, we present our findings as representing a broad range of different SDAs, which served many different types of participants, under widely varying economic conditions, within the context of local institutional arrangements that also varied substantially.

Exhibit B.1 Selected Baseline Characteristics of Adult Women: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Samples of JTPA Enrollees and Terminees

	18-month study sample	*National comparison samples*	
Characteristic	*JTPA enrollees (1)*	*JTPA enrollees (2)*	*JTPA terminees (3)*
Age [a]			
< 22	–	–	–
22 - 29	44.0%	42.6%	41.8%
30 - 44	44.0	41.5	42.1
45 - 54	7.4	8.1	7.6
> 54	4.6	7.9	8.5
Mean age	*33.2*	*33.8*	*34.4*
Ethnicity			
White, non-Hispanic	56.2%	54.0%	53.9%
Black, non-Hispanic	27.9	32.0	32.3
Hispanic	12.2	11.0	10.5
Other[b]	3.6	3.1	3.3
Education status			
HS diploma or GED certificate	73.8%	70.9%	71.7%
No HS/GED	26.2	29.1	28.3
Employment status			
Employed	15.0%	15.2%	14.9%
Not employed	85.0	84.8	85.1
Public assistance status			
Receiving any public assistance [c]	54.4%	60.2%	61.4%
Receiving AFDC	34.6	40.4	36.3
Receiving food stamps	49.6	47.8	50.1
Sample size	*2,883*	*5,032*	*5,395*

Sources: Unadjusted frequencies based on Background Information Form responses and weighted frequencies based on Job Training Quarterly Survey (JTQS) data on JTPA enrollees and terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

a. At random assignment (18-month study sample) or upon application (national comparison samples).

b. This category includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

c. "Any public assistance" includes the following sources of assistance: AFDC, food stamps, housing assistance, and other cash assistance. It does not include unemployment insurance.

Exhibit B.2 Selected Baseline Characteristics of Adult Men: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Samples of JTPA Enrollees and Terminees

	18-month study sample	*National comparison samples*	
Characteristic	*JTPA enrollees (1)*	*JTPA enrollees (2)*	*JTPA terminees (3)*
Age [a]			
< 22	–	–	–
22 - 29	45.5%	43.2%	43.4%
30 - 44	42.3	42.2	40.9
45 - 54	7.8	7.4	7.7
> 54	4.5	7.2	7.9
Mean age	*33.0*	*34.2*	*34.2*
Ethnicity			
White, non-Hispanic	57.6%	58.4%	58.0%
Black, non-Hispanic	27.6	26.2	27.1
Hispanic	10.3	11.8	11.4
Other [b]	4.5	3.6	3.5
Education status			
High school graduate or GED	70.3%	69.6%	67.8%
Neither	29.7	30.4	32.2
Employment status			
Employed	12.7%	14.3%	13.6%
Not employed	87.3	85.7	86.4
Public assistance			
Receiving any public assistance [c]	30.4%	34.9%	34.8%
Receiving AFDC	6.6	10.1	8.9
Receiving food stamps	28.5	25.8	26.5
Sample size	*2,286*	*3,835*	*4,293*

Sources: Unadjusted frequencies based on Background Information Form responses and weighted frequencies based on JTQS data on JTPA enrollees and terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

[a] At random assignment (18-month study sample) or upon application (national comparison samples).

[b] This category includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

[c] "Any public assistance" includes the following sources of assistance: AFDC, food stamps, housing assistance, and other cash assistance. It does not include unemployment insurance.

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Exhibit B.3 Selected Baseline Characteristics of Out-of-School Female Youths: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Samples of JTPA Enrollees and Terminees

	18-month study sample	*National comparison samples*	
Characteristic	*JTPA enrollees (1)*	*JTPA enrollees (2)*	*JTPA terminees (3)*
Age [a]			
16 - 19	59.3%	61.0%	60.4%
20 - 21	40.7	39.0	39.6
> 21	–	–	–
Mean age	*19.0*	*19.0*	*19.0*
Ethnicity			
White, non-Hispanic	51.2%	48.0%	48.2%
Black, non-Hispanic	28.2	37.2	34.2
Hispanic	18.8	11.7	14.9
Other [b]	1.8	3.2	2.8
Education status			
HS diploma or GED certificate	47.5%	59.1%	55.7%
No HS/GED	52.5	40.9	44.3
Employment status			
Employed	15.6%	13.3%	12.1%
Not employed	84.4	86.7	87.9
Public assistance			
Receiving any public assistance [c]	45.9%	53.0%	54.0%
Receiving AFDC	26.6	27.1	30.3
Receiving food stamps	39.6	39.5	42.1
Sample size	*1,188*	*1,725*	*1,920*

Sources: Unadjusted frequencies based on Background Information Form responses and weighted frequencies based on JTQS data on JTPA enrollees and terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

a. At random assignment (18-month study sample) or upon application (national comparison samples).

b. This category includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

c. "Any public assistance" includes the following sources of assistance: AFDC, food stamps, housing assistance, and other cash assistance. It does not include unemployment insurance.

Exhibit B.4 Selected Baseline Characteristics of Out-of-School Male Youths: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Samples of JTPA Enrollees and Terminees

	18-month study sample	*National comparison samples*	
Characteristic	*JTPA enrollees (1)*	*JTPA enrollees (2)*	*JTPA terminees (3)*
Age[a]			
16 - 19	63.9%	61.7%	63.2%
20 - 21	36.1	38.3	36.8
> 21	–	–	–
Mean age	*18.9*	*18.9*	*18.8*
Ethnicity			
White, non-Hispanic	55.2%	55.7%	54.2%
Black, non-Hispanic	26.5	28.8	27.7
Hispanic	15.8	12.2	15.8
Other[b]	2.4	3.3	2.3
Education status			
HS diploma or GED certificate	38.3%	44.7%	45.5%
No HS/GED	61.7	55.3	54.5
Employment status			
Employed	12.1%	13.7%	13.2%
Not employed	87.9	86.3	86.8
Public assistance status			
Receiving any public assistance[c]	29.2%	32.8%	32.1%
Receiving AFDC	5.7	8.3	9.0
Receiving food stamps	26.0	23.4	25.2
Sample size	*959*	*1,697*	*1,826*

Sources: Unadjusted frequencies based on Background Information Form responses and weighted frequencies based on JTQS data on JTPA enrollees and terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

a. At random assignment (18-month study sample) or upon application (national comparison samples).

b. This category includes American Indians, Alaskan Natives, Asians, and Pacific Islanders.

c. "Any public assistance" includes the following sources of assistance: AFDC, food stamps, housing assistance, and other cash assistance. It does not include unemployment insurance.

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Exhibit B.5 In-Program Experiences of Adult Women: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Sample of JTPA Terminees

	18-month study sample	*National comparison sample*
In-program experience	*JTPA enrollees (1)*	*JTPA terminees (2)*
Mean number of months enrolled	4.0	4.5
Percentage who received: [a]		
Classroom training in occupational skills	47.2%	35.2%
Basic education[b]	12.3	12.8
On-the-job training	19.5	25.0
Job-search assistance	33.6	25.8
Work experience	4.9	4.0
Miscellaneous[c]	21.1	30.4
Performance indicator		
Entered employment rate [d]	64.8%	66.2%
Mean hourly wage, if employed at termination	$5.06	$5.99
Sample size	*2,883*	*5,395*

Sources: Unadjusted enrollment and tracking data from the 16 SDAs and weighted JTQS data on JTPA terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

a. During the first formal spell of Title II-A enrollment during the 18-month follow-up period.

b. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

d. The "entered employment rate" is the percentage of all terminees who had found a job before terminating their enrollment in JTPA.

Exhibit B.6 In-Program Experiences of Adult Men: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Sample of JTPA Terminees

	18-month study sample	*National comparison sample*
In-program experience	*JTPA enrollees (1)*	*JTPA terminees (2)*
Mean number of months enrolled	3.3	3.5
Percentage who received: [a]		
Classroom training in occupational skills	26.3%	19.9%
Basic education [b]	9.1	7.2
On-the-job training	25.5	36.3
Job-search assistance	40.1	28.6
Work experience	3.0	4.0
Miscellaneous [c]	21.8	30.6
Performance indicator		
Entered employment rate	72.2%	72.2%
Mean hourly wage, if employed at termination	$5.78	$5.71
Sample size [d]	*2,286*	*4,293*

Sources: Unadjusted enrollment and tracking data from the 16 SDAs and weighted JTQS data on JTPA terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

a. During the first formal spell of Title II-A enrollment during the 18-month follow-up period.

b. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

d. The "entered employment rate" is the percentage of all terminees who had found a job before terminating their enrollment in JTPA.

Exhibit B.7 *In-Program Experiences of Out-of-School Female Youths: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Sample of JTPA Terminees*

	18-month study sample	*National comparison sample*
In-program experience	*JTPA enrollees (1)*	*JTPA terminees (2)*
Mean number of months enrolled	4.4	4.4
Percentage who received: [a]		
Classroom training in occupational skills	43.1%	30.9%
Basic education[b]	27.9	21.6
On-the-job training	14.3	20.9
Job-search assistance	34.5	20.3
Work experience	7.4	9.5
Miscellaneous[c]	21.9	32.2
Performance indicator		
Entered employment rate [d]	51.1%	57.1%
Mean hourly wage, if employed at termination	$4.43	$4.53
Sample size	*1,188*	*1,920*

Sources: Unadjusted enrollment and tracking data from the 16 SDAs and weighted JTQS data on JTPA terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

a. During the first formal spell of Title II-A enrollment during the 18-month follow-up period.

b. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

d. The "entered employment rate" is the percentage of all terminees who had found a job before terminating their enrollment in JTPA.

Exhibit B.8 In-Program Experiences of Out-of-School Male Youths: JTPA Enrollees in the 18-Month Study Sample and the National Comparison Sample of JTPA Terminees

	18-month study sample	*National comparison sample*
In-program experience	*JTPA enrollees (1)*	*JTPA terminees (2)*
Mean number of months enrolled	3.6	3.8
Percentage who received: [a]		
Classroom training in occupational skills	28.1%	19.7%
Basic education[b]	26.5	19.8
On-the-job training	19.2	27.8
Job-search assistance	36.3	19.8
Work experience	6.8	8.8
Miscellaneous[c]	26.6	36.5
Performance indicator		
Entered employment rate[d]	59.5%	60.9%
Mean hourly wage, if employed at termination	$4.77	$4.83
Sample size	*959*	*1826*

Sources: Unadjusted enrollment and tracking data from the 16 SDAs and weighted JTQS data on JTPA terminees who became enrolled in Title II-A between November 1, 1987 and September 30, 1989.

a. During the first formal spell of Title II-A enrollment during the 18-month follow-up period.

b. "Basic education" includes Adult Basic Education (ABE), high school or General Educational Development (GED) preparation, and English as a Second Language (ESL).

c. "Miscellaneous" includes assessment, job-readiness training, customized training, vocational exploration, job shadowing, and tryout employment, among other services.

d. The "entered employment rate" is the percentage of all terminees who had found a job before terminating their enrollment in JTPA.

Appendix C

Data Sources for the 18-Month Impact Analysis

THE impact analysis presented in this report is based on the experience of the 17,026 members of the experimental sample who were scheduled for a First Follow-up Survey interview 18 or more months after their random assignment—the *18-month study sample*.[1] This appendix describes five of the six data sources we used in analyzing the impacts on this sample:

- the *random assignment telephone file* compiled during the intake of the experimental sample and covering all 20,601 experimental sample members,
- *Background Information Form* responses, collected for 20,501 experimental sample members upon their application to JTPA,
- JTPA *enrollment and tracking data* provided by the 16 service delivery areas (SDAs) that served as study sites,
- First Follow-up Survey responses, collected from interviews with 17,217 members of the experimental sample,
- *earnings data from state unemployment insurance (UI) agencies* for members of the 18-month study sample in 14 of the 16 study sites, and
- *data on site characteristics* collected as part of a study of the implementation of the experiment.

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[1] Appendix D describes our definition of the study sample in more detail.

The last source, data on site characteristics, is described in Doolittle (1992), a companion report on the design and implementation of the National JTPA study.

The following sections describe each of the first five data sources in turn, reviewing the content of and method of collecting the data, the completeness of the data, and the construction of analysis variables from the data source. Detailed documentation of each source and its derivation, as well as copies of the Background Information Form and the First Follow-up Survey instrument, are available from Stephen Bell at the Bethesda offices of Abt Associates.

The Random Assignment Telephone File

The random assignment telephone file was compiled at the time JTPA applicants at the 16 study sites were randomly assigned to treatment group or control group status. Specifically, after an SDA staff member had determined a program applicant's eligibility for the program and assessed and recommended him or her for one of the three service strategies, the staff member would call The Manpower Demonstration Research Corporation (MDRC), where a computer program would randomly assign the applicant to treatment group or control group status (See Exhibit 2.1 in Chapter 2). As part of this process the computer program generated a file of identifiers and basic descriptors for the 20,601 members of the full experimental sample.

CONTENT AND COLLECTION METHOD

These identifiers and descriptors all came from information in the Background Information Form (BIF) that the applicant had completed at application. The SDA staff member read the following BIF data over the telephone: the sample member's name, Social Security number, date of birth, gender, ethnicity, recommended services and service strategy, and service delivery area (SDA). The random assignment computer program then computed four additional variables: age at random assignment (from the current date and date of birth), target group (from age, gender, and ethnicity), date of random assignment (the computer's internal date), and treatment or control group status (also generated within the computer, at random, using a 2/1, 3/1, or 6/1 treatment-control group ratio, as explained in Chapter 2). On a regular basis throughout the period of random assignment, new entries in the file were transmitted electronically to the study data base maintained at Abt Associates.

COMPLETENESS

Because all of the identifiers and descriptors for a given program applicant had to be complete for random assignment to take place, the random assignment telephone file contains no missing data.

CONSTRUCTION OF ANALYSIS VARIABLES

The random assignment telephone file was merged with the BIF file at Abt Associates before either file was edited or used to construct analysis variables. The telephone file thus served as a check on the accuracy of the information taken from the BIF (see below) and as a source for some of the variables used in constructing the impact analysis file. When in conflict, values from the BIF were accepted over values from the telephone file for all variables except those generated internally to the computer during the random assignment call, namely, the date of random assignment, and treatment-control group status.

The Background Information Form

Background Information Form responses serve as the basis for the vast majority of the baseline characteristic variables used in the impact analysis.[2] These variables are used for several purposes: to describe the characteristics of the sample; to define the target groups, service strategy subgroups, and other key subgroups examined in the analysis; and, as covariates in the impact regressions, to control for differences in baseline characteristics between the treatment and control groups.

CONTENT AND COLLECTION METHOD

The BIF provides information on sample members' demographic and household characteristics; earnings, income, and income sources, including public assistance; work education, and training histories, and other characteristics as of the time the sample members applied, to JTPA. A copy of the four-page form, which details the specific variables collected in each of these categories, appears in Bloom (1991), an earlier report in this series, and is also available from Stephen Bell at Abt Associates in Bethesda.

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[2] As noted above, two exceptions are the variables for the date of random assignment and treatment-control status, which were generated from the random assignment telephone file. A third exception is earnings data from state UI agencies, described in the last section of this appendix.

Most items on the BIF were filled out by sample members during the JTPA intake process, with help from SDA staff as needed. Three key variables were recorded directly by SDA staff, however: the SDA, the specific program, services recommended for the sample member, and the service strategy encompassing those recommended services.

The BIF also served as the SDA's record of the random assignment telephone call.[3] As noted above, all of the variables reported to MDRC during the call came from the BIF. The SDA staff entered the treatment-control group status onto the form during the call and then made a copy for SDA records. Completed forms were mailed to Abt Associates for data entry and double-key-entry verification.

COMPLETENESS

BIFs were collected for almost all experimental sample members: 20,501 of 20,601 individuals, or 99.5 percent. For 55 of the 64 variables on the form, usable data were obtained for 90 percent or more of the sample. All but 2 of the 64 variables (unemployed/not in the labor force and months since left school) had response rates for all target groups of over 80 percent.[4]

CONSTRUCTION OF ANALYSIS VARIABLES

Once entered into the Abt Associates data base, all the BIF variables were checked for out-of-range values or violations of the form's skip patterns (that is, answers to questions that should have been skipped or skips of questions that should have been answered). Unallowed values were coded to missing, and skip pattern violations were resolved either by inferring the correct answer to a question from other related responses or coding all conflicting variables to missing.

The edited data were then converted into analysis format through a series of variable construction steps. Most of these transformations involved the recording of categorical responses (such as ethnicity) into a set of dummy variables set equal to 0 or 1. More complex recodings converted whole sets of variables into the analytic concept they were intended to measure (such as the transformation of pay period, hours worked per week, and pay per pay period into the hourly wage on the most recent job). These converted variables were coded to missing when any of their source variables was missing.

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[3] The form also explained the confidentiality of the data and solicited the applicant's (or the applicant's parent's or guardian's) permission to secure information on the applicant from other public agencies.

[4] For more detail see Bloom (1991, Appendix C).

Enrollment and Tracking Data from the 16 SDAs

For standard reporting purposes JTPA service delivery areas maintain machine-readable records on all individuals enrolled in JTPA under Title II-A. These data are used in this report to identify the following variables: treatment group members who became enrolled in JTPA (*JTPA enrollees*); control group members who became enrolled in JTPA, despite the experiment's embargo on their participation (*crossovers*); the JTPA services received by both enrollees and crossovers; and the date when the enrollees were formally "terminated" from the program.

CONTENT AND COLLECTION METHOD

Data from the SDAs' management information systems show enrollment and termination dates for spells of JTPA enrollment, as well as the start and stop dates of each specific program service received during an enrollment spell. Multiple—and sometimes overlapping—services during an enrollment spell were common, whereas multiple spells of enrollment occurred on occasion but never, of course, overlapped.

Each of the SDAs in the study provided MDRC with a comprehensive, machine-readable file of all Title II-A enrollment spells that began during the sample intake period—November 1, 1987 to September 30, 1989—and extended as far as November 30, 1990, in most SDAs. MDRC staff then extracted extended enrollment information on the 20,601 experimental sample members, using the Social Security numbers that appeared on the BIFs; matches of the Social Security numbers with individuals in the SDA files were verified using the name and date of birth, where available.[5]

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[5] In the case of the Northeast (Ft. Wayne) Indiana site, SDA staff rather than MDRC staff extracted the data on sample members and then sent it to MDRC.

COMPLETENESS

By definition, the SDA data contain complete records of the formal enrollment in JTPA of all sample members in the program years covered. Hence, there are no missing records in the program participation files. As explained below, however, problems with spell dates occasionally resulted in missing data on specific dates in the file.

CONSTRUCTION OF ANALYSIS VARIABLES

The first step in constructing the analysis variables was to apply a series of date edits to any missing or inconsistent dates in the source files (such as dates of service receipt that fell outside of an enrollment spell or stop dates that preceded start dates). A small fraction of all dates emerged from this process coded as missing; data on the spells involved were excluded from the analysis.[6]

The data on enrollment spells were then converted into a series of dummy variables set equal to 0 or 1 for each of the six categories of specific program services: classroom training in occupational skills, basic education, on-the-job training, job search assistance, work experience, and miscellaneous services. Each set of variables indicates whether the sample member was enrolled in a Title II-A service in that category during the 18 months after his or her random assignment, with a separate dummy variable for each month. The algorithm used to convert the SDA service codes into the six service categories differed by site and is available from Fred Doolittle at the Manpower Demonstration Research Corporation in New York.

The First Follow-up Survey

The outcome and impact estimates in this report are based on responses to a survey of experimental sample members, conducted between November 1989 and December 1990. The First Follow-up Survey attempted to interview the 20,501 experimental sample members for whom BIF contact data were available and succeeded in interviewing 17,217. Responses to a second follow-up survey, conducted between July and December 1991, will be examined in our forthcoming final report.

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[6] A more complete description of these data preparation activities appears in Kemple, Doolittle, and Wallace. (forthcoming).

CONTENT AND COLLECTION METHOD

First Follow-up Survey responses offer a continuous history of sample members' employment and related activities over the first 13 to 39 months after their random assignment. More specifically, the survey queried respondents about spells of employment, school, and training. Selected characteristics of jobs held—including the number of hours worked, wage rate, overtime and bonus pay, and leave without pay—were collected for each job, as were the type and number of hours spent in each spell of school or training. Copies of the survey instrument, which provides details on the roughly 1,200 variables collected during each interview, are available from Stephen Bell at Abt Associates in Bethesda.

Each follow-up interview was carried out by telephone if possible or in person if not. Interviewers from the National Opinion Research Center (NORC) in Chicago used contact data from the BIF, as well as address checks through credit bureaus and other sources, to locate respondents. Names and telephone numbers of friends and relatives (from the BIF) were also used for contact purposes. Interpreters helped to conduct interviews with a small number of respondents who did not speak English.

Computer-assisted telephone interviewing, or CATI, was used for all telephone interviews. In this system, a computer program displays each question on a computer screen for the interviewer to read, and then records each answer as it is given and entered by the interviewer. CATI provides tight control over skip patterns and prompts the interviewer for corrections when out-of-range values are entered. On-site interviewers conducted in-person interviews in respondents' homes or other convenient locations. Responses to these interviews were first recorded on paper and then keyed into CATI in the NORC central office.

COMPLETENESS

The survey attempted to interview all 20,501 experimental sample members for whom contact data were available.[7] Completed interview records were obtained for 17,217 individuals, or 84 percent of the sample. This total breaks down into 9,368 telephone interviews (46 percent) and 7,849 in-person interviews (38 percent). More detailed information of survey response rates appears in Chapter 2 (see Exhibit 2.5); a discussion of the implications of nonresponse for the impact analysis appears in Appendix D.

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[7] One hundred sample members had no BIF, and hence no contact data with which to initiate an interview.

Of the variables used to compute the central employment and earnings outcomes for this report, item-specific response rates met or exceeded 90 percent in all cases.

CONSTRUCTION OF ANALYSIS VARIABLES

Several of the initial steps in survey variable construction paralleled those described above for the BIF: linkage to the telephone file, editing of out-of-range values and skip patterns, and construction of 0/1 dummy variables from selected categorical responses. A much more extensive variable construction process was used to convert data on employment and school and training spells into a set of monthly activity measures.

The first step in this process produced a set of variables indicating the share of each month spent in employment, job search, and school or training. If any dates for a spell were incomplete or inconsistent with other spell dates, all the analysis variables pertaining to that activity were coded to missing in all months. Monthly variables for earnings and employment were given the missing code for 0.9 percent of the respondents; monthly variables for school and training were given the code for 0.7 percent of the respondents.

Where the special missing code was not used, summary measures of monthly activities were constructed (hours worked, earnings, and hours of school and training) and extreme values deleted.[8] Missing or deleted values then were replaced with imputed values as described in Exhibit C.1. Where more than one spell of employment or school or training occurred within a single month, the monthly activity measures sum across spells.[9]

Selected monthly activity measures were then summed across months to produce quarterly and 18-month totals for:

- total earnings exclusive of odd-job earnings;
- total hours worked, including overtime hours but excluding odd-job hours (which were not measured);

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[8] Monthly values exceeding 347 hours of work and 250 hours of schooling were deleted, affecting 0.6 and 0.02 percent of all person-months, respectively. Earnings data were deleted in the 0.7 percent of person-months in which the ratio of earnings to hours worked (earnings per hour) fell below $.50 or exceeded $50.00 and in the person-months in which hours exceeded 347.

[9] Further details on the monthly variable construction process are available from Stephen Bell of Abt Associates.

- proportion of the time period employed, converted into weeks worked by multiplying by the number of weeks in the period;
- a 0/1 indicator of employment in the period, determined by whether hours worked (exclusive of odd jobs) equaled or exceeded zero in the period;
- total hours of classroom training in occupational skills and, separately, basic education;[10] and
- 0/1 indicators of the receipt of any classroom training in occupational skills and, separately, basic education, during the period, determined in each case by whether the corresponding hours measure equaled or exceeded zero for the period.

Where the survey data covered only a portion of the eighteenth month after random assignment, reported earnings, hours worked, and hours of school or training for that month were divided by the share of the month covered by the survey data before including them in any of these aggregate measures.

Finally, additional checks of the data were conducted to ensure that unusually large monthly and 18-month earnings values did not result from errors in the survey data. For each target group the top 1 percent of the 18-month earnings distribution was examined to determine whether any individual earnings amounts were in error.[11] So too were any individual monthly earnings amounts in excess of $5,000. These checks resulted in the discovery of 84 erroneous 18-month earnings totals among the roughly 150 cases examined.[12] Checks of cases further down the earnings distribution were not conducted due to their high cost[13] and low likelihood of detecting errors large enough, and concentrated enough in either the treatment or the control group, to noticeably affect estimates of program impact.[14] Where errors were found, we replaced each monthly earnings measure with its mean for nonerroneous data on "workers" in the same target group, service strategy subgroup, and treatment or control group.[15]

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[10] Classroom training in occupational skills includes all training received from 2- and 4-year colleges, graduate/professional schools, and vocational schools. Basic education includes high school, GED programs, adult basic education, and English as a Second Language (ESL) and other special literacy programs.

[11] This resulted in checks of earnings amounts down to $26,000 for adult women, $36,000 for adult men, $19,000 for female youths, and $24,000 for male youths on an annualized basis.

[12] Errors were identified through a detailed review of the self-reported job descriptors on which the earnings measures were based. Most errors resulted from misreported overtime or bonus earnings.

[13] Each check required a detailed examination of the timing and characteristics of all jobs reported on the survey and case-by-case determinations of the reliability of dozens of individual variables in relation to one another.

[14] We tested the sensitivity of the impact estimates to the deletion of additional earnings values and found them to be robust.

A final analysis variable taken from the survey indicates whether a sample member attained a training-related high school diploma or General Education Development certificate (GED) at any point during the follow-up period, including points more than 18 months after random assignment. This variable incorporates information from the BIF regarding the level of schooling at baseline. It was coded to "missing" if the respondent was unable to provide information on attainment of these two credentials at baseline (on the BIF) or at follow-up (on the First Follow-up Survey). It was coded to 0 if the sample member (1) already had one or both of these credentials at baseline; (2) did not report attending any school or training program of at least a week's duration during the follow-up period; and/or (3) reported attending a school or training program of at least a week's duration but did not have either credential at follow-up. The variable was coded to 1 for those who (1) lacked both credentials at baseline; (2) reported school or training program participation during the follow-up period; and (3) had one of the credentials at follow-up.

Earnings Data from State Unemployment Insurance Agencies

State agencies responsible for administering the unemployment insurance (UI) program collect quarterly data on wages and salaries for most workers. These "wage reports" are submitted by employers for individual workers, identified by their Social Security numbers (SSN). Data obtained from these systems in 14 of the 16 study sites are used to test for—and for the adult female target group, compensate for—nonresponse bias in the survey data on employment and earnings, as explained in Appendix D. Appendix E contains a comparison of the estimates obtained from these two data sources.[16]

CONTENT AND COLLECTION METHOD

Not all employers file wage reports with the UI system. Notable exceptions include federal government and railroad employers, agricultural employers, and the self-employed. Despite these omissions, an in-depth study of UI earnings data concluded recently that "the vast majority of employers are covered in all states."[17] In most cases total earnings are reported for each covered job, including wages, salaries, tips, and bonuses.

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[15] "Workers" were defined on a month-by-month basis as those members of the 18-month study sample for whom the survey indicates a positive earnings amount.

[16] Usable UI earnings data have not yet been obtained from the states of Ohio or New Jersey. Data from a third site are accurate but incomplete, covering only a portion of experimental sample members in the site. Data from this site are therefore omitted from our tests for survey nonresponse bias but included in our adjustments to remove bias for the adult women target group and in Exhibit C.2.

Abt Associates obtained UI earnings data through agreements with state UI administrators in 15 sites.[18] Data requests were submitted to each state at regular intervals, usually every six months. Those requests contained the SSNs of all experimental sample members as reported on the BIF. The states then used SSNs to extract earnings data from the wage records for the sample and return them to Abt. Most state response files contain five calendar quarters of data, with a response lag of one or two quarters.

Each response record was matched to the Abt Associates database by SSN and, where possible, by name and/or date of birth. Incomplete or flawed response files were re-requested and any problems discussed with state staff members. For each SSN states would supply zero, one, or many records for each of the quarters covered by the file, depending on the number of covered jobs held by the individual during the period in question.

COMPLETENESS

As noted earlier this report uses UI earnings data from 14 of the 16 study sites (all except those in New Jersey and Ohio), which together comprise 86 percent of the experimental sample. The data span a wide range of calendar quarters and, for most sample members, cover several quarters both before and after random assignment. The data are not complete for all sample members in all quarters, however. Because individuals were randomly assigned—and their SSNs reported to Abt Associates—over a 23-month period (November 1987 to September 1989), not all SSNs were included in the earliest data requests. Nor were all calendar quarters fully covered in any particular response file. Finally, in rare instances entire response files were unusable, so that calendar quarters covered only by those files are completely unavailable.

Aligned by quarter after random assignment, the resulting data coverage rates for the 14 sites are shown in Exhibit C.2. Only data for quarters -05 to +06 are used in the analysis, however: quarters -05 to -01 as baseline variables and quarters +01 to +04 as outcome measures in testing for survey nonresponse bias.

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[17] See Baj, Trott, and Stevens (1991).

[18] An agreement with the sixteenth site is still under negotiation.

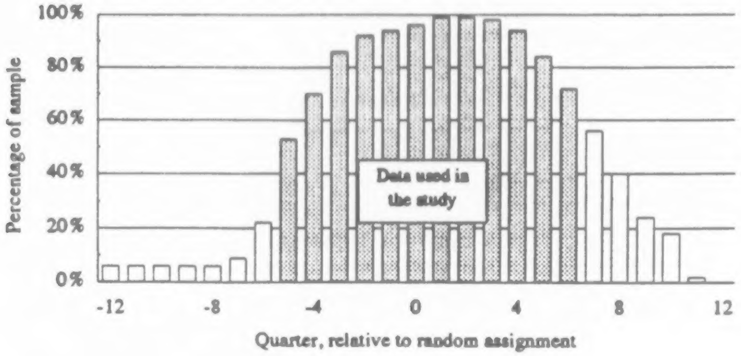
CONSTRUCTION OF ANALYSIS VARIABLES

UI earnings data for individual sample members were collapsed into a set of quarterly earnings variables through a series of four steps. First, for each response file, earnings across jobs within each quarter were summed. Second, total earnings for each quarter were extracted from the most recent response file that provided data for that quarter. Third, zero earnings were imputed for those quarters in which the state provided complete data on cases with earnings but no record for the individual in question. And finally, the calendar quarter was converted into the quarter relative to random assignment (-05 to +06).

Exhibit C.1 Imputation of Missing Earnings and Employment Variables

Imputation steps	*Percentage of person-months undergoing imputation in each step*
First step Where overtime hours, or pay, tip, and bonus earnings, and/or weeks of layoff are missing, calculate total earnings (or hours) as regular earnings (hours) times the average ratio between total and regular earnings (hours) for the rest of the sample (i.e., in months in which both are available).	4.2%
Second step Where regular hours, pay period, or pay per pay period are missing, impute total monthly hours and/or earnings as the mean of that measure across all other months with employment for that individual.	1.8%
Third step If the individual has no months with valid employment data, predict monthly hours and/or earnings from a regression equation estimated on all person-months with employment, using as regressors the respondent's baseline characteristics, characteristics of the most recent job in the follow-up period, and time since random assignment.	2.1%
Fourth step Missing hours in school or training are imputed as the sample mean for months with school or training.	0.2%

Exhibit C.2 Coverage Rates for Earnings Data from State UI Agencies, by Quarter, relative to Random Assignment



Source: Earnings data from state UI agencies.
Note: No UI data were obtained from New Jersey and Ohio. This exhibit applies to the 14 sites located outside of these states.

Appendix D

Sample Definition and Impact Estimation Methods

THIS appendix specifies the methods used to analyze outcomes and impacts in Chapters 4 through 7, the Executive Summary, and Appendix H. Sections 1-4 provide information, definitions, and model specifications basic to the analyses of all target groups; Sections 5-8 specify the methods used to estimate impacts on adult men and youths; Section 9 presents a test for survey nonresponse bias; and Section 10 describes how we modified the methods of Sections 5-8 to estimate impacts on adult women. Section 11 specifies the tests reported in Exhibit H.22 that consider whether impacts on the earnings of youths deviate significantly from a linear impact-versus-age trend established by estimated impacts on the earnings of adults in their twenties.

1. THE 18-MONTH STUDY SAMPLE

The outcome measures for this report are taken from the First Follow-up Survey. Because the survey was conducted over a 12-month period, while random assignment occurred over a 23-month period, the scheduled length of follow-up after random assignment varied from 13 to 23 months; the actual length of follow-up varied somewhat more because of time lags in locating and interviewing some sample members. In order to maintain a constant sample over the period analyzed in this report, we defined the 18-month study sample to include only those members of the experimental sample who were scheduled to be interviewed at least 18 months after random assignment. (We used the scheduled, not the actual, interview date because treatment or control status is independent of the scheduled date.) This restriction excluded 3,266 persons, or 15.9 percent of the full experimental sample of 20,601.

We also excluded certain experimental sample members who were randomly assigned at a different treatment-to-control ratio than the majority of the sample. The standard ratio was 2/1. During the course of random assignment, however, five SDAs which had difficulty recruiting JTPA applicants were allowed to increase the treatment-to-control ratio temporarily to 3/1 or 6/1 for specific groups. To preserve the balance between the treatment and control groups, we randomly selected and excluded from the analysis one-third of those treatment group members assigned using a 3/1 ratio and two-thirds of those treatment group members assigned using a 6/1 ratio. This procedure excluded 473 persons, or 2.3 percent of the full experimental sample.

Finally, we excluded five experimental sample members in Oakland who, according to our records, were under age 22 at random assignment. As the Oakland SDA excluded youths from the study, these persons either were not intended to be included or were older than our records indicate.

The resulting 18-month study sample includes 17,026 persons, or 82.6 percent of the full experimental sample.

2. SUMMARY OF DATA COMPLETENESS

A detailed description of the data sources used in this report is given in Appendix C. As noted above, the outcome measures are taken from the First Follow-up Survey. The other principal data sources for this report are the Background Information Form (BIF) and earnings data from state Unemployment Insurance (UI) agencies. Appendix E provides a comparison of the UI and survey data on earnings.

The overall completion rate for the BIF was 99.5 percent; the follow-up survey response rates by target group and treatment or control status were:

	Treatment	Control
Adult Women	88.3%	87.8%
Adult Men	80.8%	79.2%
Female Youths	88.3%	86.5%
Male Youths	84.3%	79.5%

In addition to the unit nonresponse of those sample members for whom there was no completed BIF or no follow-up interview, there was some item nonresponse on the completed forms and interviews. Item nonresponse on the BIF was generally less than 5 percent.

Nine-tenths of one percent of the respondents to the follow-up survey provided insufficient information to determine employment status in all months of the follow-up period. Because of the low frequency of the problem and the complexity of attempting to salvage information on the months for which employment status could be determined, we did not use the survey data on the employment and earnings of these persons.[1]

When employment status in each month was known but hours worked and/or earnings in some or all months could not be determined, the missing data were imputed as described in Appendix C. Such imputations were made for 8 percent of all person-months.

3. FRAMEWORK FOR STATISTICAL INFERENCE

Our significance tests are based on the assumption that the 18-month study sample is a simple random sample from a much larger population of interest.[2] We report tests of null hypotheses about the impact (defined below) of assignment to the treatment group on post-assignment outcomes in this population.

To define impact, we need to consider two hypothetical outcomes, of which at most one is realized for any given person. Suppose we are interested in earnings during some period after random assignment. Let y_i^T denote the amount that person i would earn if assigned to the treatment group; let y_i^C denote the amount she would earn if assigned to the control group.[3]

The *treatment and control means* of y (or treatment and control levels of mean y) in the population are the population means of y_i^T and y_i^C, respectively. The *impact* of assignment to the treatment group on mean earnings in the population (or impact per assignee) is the difference between the treatment and control means.

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[1] These were generally cases in which the start and/or stop date of one or more employment spells was missing or inconsistent. If, for example, the start date of an employment spell is missing, then it is not possible to determine employment status in *any* month prior to the stop date of the employment spell.

[2] An alternative framework, randomization theory, would take the 18-month study sample itself as the population of interest. In this framework, the only element of chance is random assignment. When the number of units randomly assigned is large, the distributions of conventional test statistics under randomization theory may not differ appreciably from their distributions under sampling theory (e.g., Scheffé 1959, pp. 313-24).

[3] We assume that person i's values of y_i^T and y_i^C are not affected by the assignment of other persons to the treatment and control groups. Rubin (1980) calls this the "stable-unit-treatment-value assumption" (SUTVA).

The definitions above are appropriate for both continuous and binary outcomes. If y denotes employment status instead of earnings, with values of 1 for employed and 0 for not employed, then the treatment and control levels of percentage employed (mean employment status) are the population means of y_i^T and y_i^C. The impact of assignment to the treatment group on percentage employed is the difference between the treatment and control levels.

Sections 4 and 5 explain how we estimate treatment and control means and impacts per assignee and test the null hypothesis of zero impact.

4. REGRESSION MODELS: FUNCTIONAL FORMS AND REGRESSORS

Unbiased and consistent estimates of treatment and control means and impacts could be obtained by simply computing sample mean outcome levels in the treatment and control groups and taking their difference. We instead use regression-adjusted means in most of our analyses (as indicated in exhibit footnotes) in order to increase the statistical precision of our impact estimates and the power of our significance tests. This section specifies the models and regressors used; Section 5 gives the details of the basic estimation methods.

We use linear models to estimate treatment and control levels of and impacts on mean earnings, hours worked, and weeks worked; we use logistic models to estimate treatment and control levels of and impacts on percentage employed. In the notation of Section 3, the observed outcome, y_i, is equal to y_i^T if i is a treatment group member and y_i^C if i is a control group member. The linear models assume that the expectation of y_i, conditional on the regressors, $\mathbf{x}_i$, is a linear function of $\mathbf{x}_i$. The logistic models assume that $\log\,[p_i\ /\ (1 - p_i)]$ is a linear function of $\mathbf{x}_i$, where p_i is the probability that $y_i = 1$ (i is employed), conditional on $\mathbf{x}_i$.

The regressors used with each target group consist of a constant, a dummy variable for assignment to the treatment group, and a set of baseline covariates shown in Exhibit D.1. Where the value of a baseline covariate is missing, we insert the target group mean.

5. BASIC IMPACT ESTIMATION METHODS

This section specifies the methods used to estimate impacts per assignee on most outcomes for adult men, female youths, and male youths and for the service strategy subgroups and two-year age groups within those target groups. The extensions used to estimate impacts on the components of earnings and on the mean earnings of other subgroups are described in Sections 7 and 8, respectively; the derivation of estimated impacts per enrollee is explained in Section 6. Section 10 describes how we modified these methods to compensate for the apparent survey nonresponse bias in the data on adult women.

A. Mean Earnings, Weeks Worked, and Hours Worked

In the analyses of mean earnings, hours worked, and weeks worked, we use ordinary least squares to estimate the parameters of the linear model specified in Section 4. The regression sample consists of all survey respondents in the target group who provided sufficient information to determine employment status in all months of the follow-up period. The estimated coefficient on the treatment group dummy variable is our estimate of impact per assignee; the two-tailed *t* test for that coefficient is our test of the null hypothesis of zero impact. We estimate the control mean by substituting the target group mean covariate values into the estimated model and setting treatment to zero. (In calculating the mean covariate values, we include both survey respondents and nonrespondents.) The estimated treatment mean is the sum of the estimated control mean and impact.

We estimate separate regressions for each month, for each quarter, and for the 18-month period after random assignment.

We estimate treatment and control means and impacts for the two-year age groups (Exhibit 6.3) and the service strategy subgroups by estimating a separate regression for each of these subgroups, omitting some of the regressors in Exhibit D.1 when necessary to avoid multicollinearity. We estimate the control mean by substituting the mean covariate values for the subgroup into the estimated model.

B. Percentage Employed

In the analyses of percentage employed, we use maximum likelihood to estimate the parameters of the logistic model specified in Section 4. The regression sample is the same as described in part A of this section. Again, the two-tailed *t* test for the coefficient on treatment is our test of the null hypothesis of zero impact. The estimation of treatment and control levels and impact is more complicated, however. Because the logit model is nonlinear, the probability evaluated at the mean values of the covariates does not equal the mean of the individual probabilities. If we substituted the target group mean covariate values into the estimated logit model and set treatment to zero, we would not get a prediction of what the control group employment rate would be if the treatment and control groups had identical baseline characteristics. We would get a prediction of what the control group employment rate would be if every control group member had covariate values equal to the mean. In a linear model, these two predictions are the same, but in a nonlinear model, they generally differ.

Because the transformation $e^x / (1 + e^x)$, which converts log odds ratios to probabilities, is concave for $x > 0$ and convex for $x < 0$, substituting the mean covariate values into the estimated logit model would tend to give upward biased estimates of rates above 50 percent (positive log odds ratio) and downward biased estimates of rates below 50 percent (negative log odds ratio).

We therefore adopt the following procedure (Lane and Nelder 1982) to estimate treatment and control levels of and impacts on percentage employed. For each person i in the target group, we use the estimated logit model and i's covariate values to calculate two predicted probabilities: $\hat{p}_i^T$, the probability that i would be employed if assigned to the treatment group; and $\hat{p}_i^C$, the probability that i would be employed if assigned to the control group. Our estimates of the treatment and control levels of percentage employed are the means of $\hat{p}_i^T$ and $\hat{p}_i^C$, respectively, over the target group.[4] The difference between the estimated treatment and control levels is our estimate of the impact of assignment to the treatment group on percentage employed.

C. *Distribution of 18-Month Earnings*

The analyses of impacts on the distribution of 18-month earnings for adult men (Exhibit 5.4) and [illegible]-of-school youths (Exhibits H.2, H.8, and H.15) rely on unadjusted frequencies. The nonzero earnings categories are approximate quartiles of the earnings distribution of those control group members who had positive earnings. For each of the five earnings categories, we report a two-tailed t test of the null hypothesis of zero impact on the proportion in that category. We also report a chi-square test of the null hypothesis of no impact on the overall distribution (e.g., Snedecor and Cochran 1989, pp. 202-03).

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[4] From the first-order conditions for maximizing the likelihood function of the logit model, the mean of $\hat{p}_i^T$ (or $\hat{p}_i^C$) over all treatment (or control) group members in the regression sample is the unadjusted treatment (or control) group employment rate. By taking means over the full target group, we adjust for chance treatment-control differences in baseline characteristics.

D. Attainment of a Training Related High School Diploma or GED Certificate

The estimated rates of training-related high school diploma or GED receipt (Exhibits S.5, S.11, 4.9, 5.8, 6.5, 6.10, 7.5, and H.18) are unadjusted percentages. Our two-tailed *t* test of the null hypothesis of zero impact is derived from the unadjusted frequencies within the high school dropout subgroup (those who had neither credential at the time of application to JTPA); if the null hypothesis of zero impact on this subgroup is rejected, we infer a rejection of the null hypothesis for the full sample. Thus, our significance levels for the full sample are always the same as for the dropout subgroup.

E. Month of First Job (Out-of-School Youths)

The "month of first job" for out-of-school youths (Exhibit H.23) is defined as follows. If the person was employed at any time during the period between the random assignment date and the end of the month of random assignment, we let the month of first employment equal zero. If the first job after random assignment began during the *x*th month after random assignment, with $1 \le x \le 18$, we let the month of first employment equal *x*. If the person was never employed during the follow-up period, we let the month of first employment equal 18. Exhibit H.23 reports unadjusted mean values of this variable. We report the standard two-tailed *t* test for the comparison of the means of two independent samples.

6. IMPACTS PER ENROLLEE: ADJUSTMENTS FOR TREATMENT GROUP NONENROLLEES AND CONTROL GROUP CROSSOVERS

For purposes of exposition, we present the adjustment for nonenrolled treatment group members (nonenrollees) first. The estimates of impact per enrollee in this report are, however, simultaneously adjusted for nonenrollees and crossovers, as explained in part B of this section. The estimates of impact per assignee are not adjusted.

A. Adjustment for Nonenrollees

Estimates of impact per assignee do not measure the effect of JTPA on those who actually enrolled, because 33.2 to 39.2 percent of treatment group members in each target group did not enroll in JTPA during the first 18 months after random assignment. To estimate the effect of the program on those who did enroll, we use an adjustment proposed by Bloom (1984a).[5]

The impact per assignee is a weighted average of the impact on those who would enroll if assigned to the treatment group and the impact on those who would not enroll, with weights r and $1 - r$, where r is the proportion who would enroll. If we assume that the impact on those who would not enroll is zero, then the impact per assignee is r times the impact on those who would enroll. Therefore, to obtain a consistent estimate of the impact on persons who would enroll if given the opportunity, we can simply divide the estimated impact per assignee by the treatment group enrollment rate.

Note that the only assumption required for this adjustment is that assignment to the treatment group has zero average impact on persons who would not enroll. It is not necessary to make any assumption about whether enrollees are typical of assignees.

Unfortunately, the assumption of zero impact on nonenrollees is not innocuous. Appendix F describes typical SDA practices regarding formal enrollment in JTPA and reports the results of a survey of 307 nonenrolled treatment group members. Roughly half of this sample received some JTPA services, although these services were typically much more limited than those received by enrollees.

Under certain conditions, the estimates of impact per enrollee and per assignee will estimate upper and lower bounds on the true impact on enrollees. If the true impact on nonenrollees is of the same sign as the true impact on enrollees, then the magnitude of the estimated impact *per enrollee* will be an estimate of an *upper bound* on the magnitude of the true impact on enrollees. If the true impact on nonenrollees is smaller in magnitude than the true impact on enrollees, then the magnitude of the estimated impact *per assignee* will be an estimate of a *lower bound* on the magnitude of the true impact on enrollees. These results follow directly from the expression of the impact per assignee as a weighted average of the impact on enrollees and the impact on nonenrollees.

B. Simultaneous Adjustment for Nonenrollees and Crossovers

Between 2.0 and 3.8 percent of control group members in each target group enrolled in JTPA during the first 18 months after random assignment. Because of these "crossovers," average outcome levels in the control group are not unbiased estimates of what outcome levels would have been for treatment group members if they had been denied JTPA services.

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[5] The same adjustment was proposed by Tarwotjo et al. (1987) and Sommer and Zeger (1991) to estimate the impact of a therapeutic agent on those who comply with their regimen in a randomized clinical trial. Sommer and Zeger's restriction of their proposal to binary outcomes is unnecessary.

To adjust for the presence of control group crossovers as well as treatment group nonenrollees, we used an extension of the adjustment for nonenrollees (Bloom 1985). This extension is based on two additional assumptions. First, we assume that all "crossover-type" persons (those who would enroll if assigned to the control group) would also enroll if assigned to the treatment group. Second, we assume that, in the notation of Section 3, y_i^T and y_i^C are equal for crossover-type persons. The impact of assignment to the treatment group on crossover-type persons is then zero.

The impact per assignee is a weighted average of the impacts on three groups: (1) those who would enroll if assigned to the treatment group but would not if assigned to the control group; (2) those who would never enroll; and (3) crossover-type persons. The weights on the three groups are $r - c$, $1 - r$, and c, respectively, where r is the proportion who would enroll if assigned to the treatment group and c is the proportion of crossover-type persons. Under the assumptions that the impacts on groups (2) and (3) are zero, the impact per assignee is $r - c$ times the impact on group (1). Therefore, to obtain a consistent estimate of the impact on group (1), we divide the estimated impact per assignee by the difference between the treatment and control group enrollment rates within the target group or subgroup under study. These enrollment rates are the percentages enrolled in JTPA anytime during the first 18 months after random assignment.

This adjustment does not require any assumption that the crossovers are typical of the control group. It should be noted, however, that the adjusted impact estimate does not apply to enrollees as a whole, but to the slightly narrower population of non-crossover-type enrollees.

Because the crossover rate is low, alternative methods of addressing the crossover problem would not change the estimates substantially.

In Exhibit S.1, estimated impacts on earnings per enrollee are expressed in both dollar and percentage terms. The denominator for the percentage calculation is the difference between the unadjusted mean earnings of treatment group enrollees in the target group and the estimated impact per enrollee. This denominator is an estimate of what enrollees would have earned in the absence of the program.

7. PERCENTAGE IMPACTS ON THE COMPONENTS OF EARNINGS: ESTIMATION METHODS

If an individual is employed at all during a given period, her earnings during that period can be expressed as the product of three components: weeks worked, hours worked per week worked, and earnings per hour worked. Letting y_i denote earnings, h_i hours worked, and w_i weeks worked, this statement is simply the equation

$$y_i \;=\; w_i\,(h_i/w_i)\,(y_i/h_i)\;.$$

Analogously, a group's mean earnings can be expressed as the product of four components. Let Y, H, and W denote mean earnings, mean hours worked, and mean weeks worked, respectively, with zeros included. Let M denote the proportion of the group employed at all during the period. We can express Y as the product of four components:

$$Y \;=\; M\,(W/M)\,(H/W)\,(Y/H)\;.$$

When percentage impacts on these four components are small, the percentage impact on mean earnings is approximately equal to the sum of the percentage impacts on the components. The first component is simply the percentage employed. The second component is mean weeks worked for persons who worked. The third component, mean hours worked divided by mean weeks worked, is not necessarily equal to the mean of hours per week (h_i/w_i) for persons who worked. Rather, it is a weighted mean of hours per week for persons who worked, with weights proportional to weeks worked. Similarly, the fourth component, mean earnings divided by mean hours worked, is not necessarily equal to the mean of hourly earnings (y_i/h_i) for persons who worked; it is a weighted mean with weights proportional to hours worked.

In Exhibits 4.7, 4.14, 5.6, 5.13, 6.8, 6.13, H.4, H.10, and H.17 we refer to Y (mean earnings during the 18-month period) as earnings per assignee and to M, W/M, H/W, and Y/H as workers per assignee, weeks worked per worker, hours worked per week worked, and earnings per hour worked, respectively.

We derive estimates of percentage impacts on the four components from the estimated treatment and control means of employment status, weeks worked, hours worked, and earnings (denoted here by $\hat{M}^T$ and $\hat{M}^C$, $\hat{W}^T$ and $\hat{W}^C$, $\hat{H}^T$ and $\hat{H}^C$, and $\hat{E}^T$ and $\hat{E}^C$).

The estimated percentage impact on workers per assignee is $\dfrac{\hat{M}^T}{\hat{M}^C} - 1$.

The estimated percentage impact on weeks worked per worker is $\dfrac{\hat{W}^T \,/\, \hat{M}^T}{\hat{W}^C \,/\, \hat{M}^C} - 1.$

The estimated percentage impact on hours worked per week worked is $\dfrac{\hat{H}^T \,/\, \hat{W}^T}{\hat{H}^C \,/\, \hat{W}^C} - 1.$

The estimated percentage impact on earnings per hour worked is $\dfrac{\hat{Y}^T \,/\, \hat{H}^T}{\hat{Y}^C \,/\, \hat{H}^C} - 1.$

Exhibits S.2 and 7.7 present a decomposition of estimated percentage impacts on earnings per assignee into estimated percentage impacts on *two* components: hours worked per assignee and earnings per hour worked. This two-component decomposition is completely analogous to the four-component decomposition described above. The estimated percentage impact on earnings per hour worked is the same as in the four-component decomposition. The estimated percentage impact on hours worked is simply the ratio of the estimated impact on mean hours worked to the estimated control mean.

8. Impacts on Earnings, by Key Subgroup and by Study Site

This section explains how subgroups within each target group were formed and then describes the derivation of the subgroup impact estimates and t and F tests reported in Exhibits S.7, S.8, S.13, S.14, S.15, 4.15, 5.14, 6.14, 6.15, 7.8, and H.21. Finally, the analysis of impacts by study site in Chapter 7 is described.

A. Impacts on Key Subgroups Within Each Target Group

With the exception of the arrest history subgroups of the out-of-school youth target groups, all subgroups were defined using information from the random assignment telephone file or the Background Information Form. The arrest history subgroups were defined using responses to a First Follow-up Survey question about arrests before random assignment. When information on a relevant variable was unavailable for certain persons, those persons were not included in any subgroups for which the definitions relied on that variable. For example, persons who did not report marital status were not included in any of the household composition subgroups.

In Exhibits 4.15, 5.14, 6.14, 6.15, and H.21, we present one set of estimated control means and three sets of subgroup impact estimates. Column (2) gives the unadjusted mean earnings of control group members within each subgroup. Column (3) gives estimates of the impact per assignee on earnings of each subgroup; these estimates are also reported for selected subgroups in Exhibits S.7, S.8, S.13, S.14, S.15, and 7.8.

Column (4) gives model-based extrapolations that estimate what the impact on each subgroup would be if the subgroup had the same distribution across sites as the full target group. Analogously, column (5) estimates what the impact on each subgroup would be if the subgroup had the same distribution across sites and service strategies as the full target group. Columns (4) and (5) are shown to examine the question of whether differences between subgroup impact estimates in column (3) are due to differences in distribution across sites and/or service strategies.

To compute the impact estimates shown in column (3), we estimate one regression for each set of complementary subgroups (e.g., the three ethnicity subgroups). Defining a dummy variable for membership in each subgroup, we regress earnings in the first 18 months after random assignment on the subgroup dummy variables, the interactions of treatment with the subgroup dummy variables, and the baseline covariates in Exhibit D.1. (The uninteracted treatment group dummy variable is omitted to avoid multicollinearity. Certain baseline covariates are also omitted when necessary.) The impact estimates shown in column (3) are the estimated coefficients on the treatment × subgroup interactions. The two-tailed *t* test on each of these coefficients is our test of the null hypothesis of zero impact on mean earnings of the appropriate subgroup. Each subgroup impact estimate shown in column (3) converges asymptotically to the same limit as the difference in mean earnings between treatment and control group members within the subgroup.

To test the null hypothesis that impacts on complementary subgroups do not differ, we perform an *F* test by estimating a supplementary regression in which the treatment × subgroup interactions are replaced by the uninteracted treatment group dummy variable. This regression restricts the impacts on the subgroups to be equal. As usual, the *F* test compares the sums of squared residuals from the restricted and unrestricted regressions.

The procedure used to produce the impact estimates shown in column (4) is equivalent to a regression of earnings on the regressors used for column (3) and interactions of treatment with a full set of site dummy variables (omitting one treatment × site interaction to avoid multicollinearity). For each subgroup, the impact estimate shown in column (4) can be computed by cross-multiplying the estimated coefficients on the treatment × site interactions with the target group means of the site dummy variables and adding the estimated coefficient on the treatment × subgroup interaction. To facilitate the derivation of a *t* statistic for this estimate, we use an equivalent procedure in which treatment is interacted with the deviations of the site dummy variables from their target group means. The impact estimate described above is then simply the coefficient on the treatment × subgroup interaction, and its *t* statistic is computed automatically by the regression software. We again perform an *F* test for each set of complementary subgroups by estimating a supplementary regression in which the treatment × subgroup interactions are replaced by the uninteracted treatment group dummy variable.

To compute the impact estimates shown in column (5) and the corresponding *t* and *F* statistics, we use an analogous procedure, replacing the treatment × site interactions with treatment × site × service strategy interactions.

B. *Analysis of Impacts by Study Site*

The impact estimates and tests reported in Exhibits 7.9 and 7.10 are derived by the same procedure as the estimates and tests reported in column (3) of the exhibits on key subgroups within each target group. Here, the sites are treated as subgroups and site-specific impacts are estimated using the treatment × site interactions.

Additional analyses of impacts by site involved the calculation of correlation coefficients across target groups (for Exhibit 7.11) and ordinary least squares regressions of total earnings during the 18-month period on a set of covariates and treatment × covariate interactions described in Chapter 7 (see discussion of Exhibit 7.12). The usual formulas were used to calculate the correlation coefficients, treating each site as an observation.

9. TEST FOR SURVEY NONRESPONSE BIAS

The overall response rate for the First Follow-up Survey was 84.8 percent, with target group response rates ranging from 80.3 to 88.2 percent. We tested for survey nonresponse bias in the impact estimates, using Unemployment Insurance (UI) earnings data for both survey respondents and nonrespondents in 13 of the 16 study sites (excluding Butte, Montana; Jersey City, New Jersey; and Marion, Ohio). To construct a test, we used two subsets of the 18-month study sample to estimate impacts on the sum of UI-reported earnings over the first four calendar quarters after random assignment: (1) all members with complete UI earnings data for calendar quarters 1-4; and (2) all members in the first group who also had complete survey earnings data for months 1-18. These impact estimates were produced by the estimation method described in Section 5, a linear regression of total UI-reported earnings over calendar quarters 1-4 on a treatment group dummy variable and the regressors in Exhibit D.1. The difference between the two impact estimates is an estimate of the bias introduced by restricting the analysis to survey respondents.

The estimated bias, by target group, is given below.

Estimated Bias per Year	
Adult Women	$86
Adult Men	−$26
Youths	−$38

The estimated biases for adult men and youths were judged acceptable, while the bias for adult women was deemed unacceptable. Therefore, in the analysis of impacts on the earnings and employment of adult women, we applied imputation procedures described in the next section.

The nonresponse bias for adult women appears to be concentrated in the treatment group. The unadjusted means of total UI-reported earnings over calendar quarters 1-4, by treatment or control status and response or nonresponse, were:

	Treatment	Control
Respondents	$4,154	$3,695
Nonrespondents	$2,950	$3,481

10. ADJUSTMENTS FOR SURVEY NONRESPONSE BIAS IN THE DATA ON ADULT WOMEN

This section specifies the imputation procedures used for the adult female target group in estimating treatment and control levels and impacts on mean earnings, the distribution of earnings, percentage employed, mean weeks and hours worked, and the components of earnings. The general approach was to use Unemployment Insurance (UI) earnings data to impute individual or mean outcome values for survey nonrespondents (and for respondents who provided insufficient information to determine employment status in all months) in 14 of the 16 study sites (excluding Jersey City, New Jersey, and Marion, Ohio).[6] At the end of this section, estimates based on the imputations are compared with estimates that use data on respondents only.

A. Mean Earnings

To impute earnings for nonrespondents, we first estimated 18 linear regressions (one for each month) with the subset of adult female respondents that had UI earnings data covering any of the first six calendar quarters after random assignment. In this subsample, we regressed survey-reported earnings on a constant and 18 additional regressors, three for each of calendar quarters 1-6: a dummy variable indicating whether UI-reported earnings were missing; a dummy variable indicating whether UI-reported earnings were greater than $5,000; and a continuous variable set equal to zero if UI-reported earnings were missing or greater than $5,000 and set equal to UI-reported earnings otherwise.[7] We used the estimated coefficients from these regressions to predict the missing values of survey-reported earnings in each month for survey nonrespondents who had UI earnings data covering any of calendar quarters 1-6. The nonrespondents were then included, with these predicted values, together with the respondents in linear regressions of the form specified in Sections 4 and 5. To protect our significance tests for impacts on mean earnings against any conditional heteroskedasticity introduced by the imputations, we used the White (1980) standard error estimator.

B. Distribution of 18-Month Earnings

In Exhibit 4.5, the nonzero earnings categories are approximate quartiles of the earnings distribution of those respondents in the control group who had positive earnings. We added imputed frequencies of nonrespondents to unadjusted frequencies of respondents. The imputed frequencies of nonrespondents were derived by the following method:

We divided the range of total UI-reported earnings over calendar quarters 1-5 into thirteen intervals: $0; eight $1,000-width intervals (from $1-1,000 to $7,001-8,000); three $2,000-width intervals (beginning at $8,001; $10,001; and $12,001); and $14,001 or more.

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[6] Because the UI records from Montana appeared accurate but covered only a subset of all sample members in the site, we included Montana in these imputations but not in the test for nonresponse bias.

[7] The number of adult women with UI-reported earnings exceeding $5,000 in a quarter ranged from 23 in the first quarter to 96 in the sixth quarter. The number of observations in each regression was 4,965, or about 85% of all adult female respondents. The R^2 ranged from .35 to .42, except in the regression for the first month, which had an R^2 of .26.

Examining the set of persons for whom both survey and UI earnings data were available, we produced a 13 × 5 contingency table of total UI-reported earnings over calendar quarters 1-5 by total survey-reported earnings over quarters 1-6, using the 13 UI earnings categories listed above and the 5 survey earnings categories shown in Exhibit 4.5. We used this contingency table and the distributions of UI-reported earnings for nonrespondents in the treatment and control groups to predict how many nonrespondents in each of the two groups would fall into each of the five survey earnings categories.

The t and chi-square tests reported in Exhibit 4.5 treat the adjusted frequencies as if they were unadjusted.

C. Percentage Employed, Mean Weeks and Hours Worked, and the Components of Earnings

To impute employment status for nonrespondents, we first estimated seven logistic regressions (one for each quarter and one for the 18-month period) with the subset of adult female respondents that had UI earnings data covering any of the first six calendar quarters after random assignment. In this subsample, we regressed survey-reported employment status on a constant and 12 additional regressors, two for each of calendar quarters 1-6: a dummy variable indicating whether UI-reported earnings were missing and a dummy variable indicating nonzero UI-reported earnings. We used the estimated coefficients from these regressions to calculate predicted probabilities of employment in each period for survey nonrespondents who had UI earnings data covering any of calendar quarters 1-6. A random number generator was used to impute employment status in each period for these nonrespondents. The nonrespondents were then included, with these imputed values, together with the respondents in logistic regressions of the form specified in Sections 4 and 5.

Adjustments for nonresponse bias were then extended to estimates of impact on weeks and hours worked, which could not be observed in the UI data. Here, we applied adjustments that make estimated percentage impacts on mean weeks and hours worked compatible with the estimated percentage impacts on employment status and earnings.[8] We began by using the formulas shown in Exhibit D.2.

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[8] We refer here to percentage impacts on weeks and hours worked averaged across *all* sample members, including those persons who did not work during the follow-up period. As described below, later steps in the procedure convert these percentages into natural units (weeks and hours) and use them to calculate percentage impacts on weeks worked per worker and hours worked per week.

The formulas in Exhibit D.2 were designed for the cases in which b and c lie between a and d: that is, in the respondent-based set of estimates, the percentage impacts on weeks and hours worked lie between the percentage impact on percentage employed (the impact on percentage employed expressed as a percentage of the control level) and the percentage impact on earnings. The formulas then use the relative positions of b and c in the interval between a and d to impute the relative positions of the nonresponse-adjusted estimates of percentage impact on weeks and hours worked in the interval between x and z (the nonresponse-adjusted estimates of percentage impacts on employment and earnings). This approach translates the progression of growing (or declining) percentage effects on employment, weeks worked, hours worked, and earnings for respondents (a, b, c, d) into a matching progression between two different nonresponse-adjusted endpoints (x and z).

The formulas in Exhibit D.2 are not appropriate when b or c lies outside the interval bounded by a and d. In such cases, we allowed the corresponding nonresponse-adjusted estimate to lie above or below the interval bounded by x and z by the same percentage point amount. Thus, for example, if $c > d > a$, then the nonresponse-adjusted estimate of percentage impact on hours worked was set to $z + (c - d)$.

A third procedure was used when $d - a$ and $z - x$ took opposite signs. This occurred only for the other services strategy subgroup in quarters 1 and 2. In this instance, we concluded that the positions of b and c relative to a and d in those quarters were of no use in imputing nonresponse-adjusted estimates of impact on weeks and hours worked. We imputed estimates of impact on weeks and hours worked in those quarters by subtracting the sum of the nonresponse-adjusted impact estimates for quarters 3-6 (which were all derived by the first or second procedure) from the nonresponse-adjusted estimate of impact for the entire follow-up period (which was derived by the second procedure) and dividing the difference evenly between quarters 1 and 2.

We estimated the control mean of weeks worked by multiplying the respondent-based control mean of weeks worked by the ratio of the nonresponse-adjusted control level of percentage employed to the respondent-based control level of percentage employed. To estimate the control mean of hours worked, we multiplied the respondent-based control mean of hours worked by the ratio of the nonresponse-adjusted control mean of weeks worked to the respondent-based control mean of weeks worked. We then derived estimates of treatment means of and impacts on weeks and hours worked from the new control means and the nonresponse-adjusted estimates of percentage impact described above. Our significance tests for the nonresponse-adjusted estimates of impacts on weeks and hours worked use the estimated standard errors for the respondent-based estimates.

Finally, we derived estimates of impacts on the components of earnings by applying the procedures described in Section 7 to the nonresponse-adjusted treatment and control means.

D. *Comparison of Results With and Without Nonrespondent Imputations*

Exhibit D.3 shows two sets of estimated control levels of and impacts on mean earnings of adult women. The estimates in the top panel use data on respondents only. The estimates in the bottom panel, which are also presented in Exhibits 4.3 and 4.4, are derived by the procedure described in part A of this section.[9] Note that although the two estimates of the control mean of total earnings over the follow-up period differ by only $19, the respondent-based estimate of impact over the follow-up period is $106 higher than the nonresponse-adjusted estimate. The nonresponse bias adjustment lowers the treatment mean but hardly changes the control mean because, as reported in Section 9, UI-reported earnings of nonrespondents in the treatment group are substantially lower than those of treatment group respondents, but UI-reported earnings of respondents and nonrespondents in the control group are similar.

Exhibit D.4 shows the respondent-based and nonresponse-adjusted estimates of control levels of and impacts on percentage employed, mean weeks worked, and mean hours worked. The nonresponse bias adjustments tend to lower the impact estimate, but the effect on the control mean varies.

11. TEST FOR DEVIATIONS OF IMPACTS ON YOUTH COHORTS FROM TREND IN YOUNG ADULT IMPACTS

As noted in Section 5, the estimated treatment and control means and impacts for the two-year age groups shown in Exhibit 6.3 are derived from split-sample regressions. Exhibit H.22 reports the results of tests designed to consider whether impacts on the youth age groups deviate significantly from a linear impact-versus-age trend established by the adult age groups. We perform these tests separately for females and males. We estimate the following equation, pooling all 18-month study sample members of the relevant gender who were age 16 to 29 at random assignment:

$$y_i = a + \mathbf{b}'\mathbf{x}_i + cA_i + dK_i + fL_i + gM_i + T_i(p + qA_i + \alpha K_i + \beta L_i + \gamma M_i) + \epsilon_i,$$

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[9] All significance tests reported in Exhibit D.3 rely on the White (1980) standard error estimator, although use of the conventional estimator would not have altered significance levels appreciably.

where

y_i = total earnings during the first 18 months after random assignment;

$\mathbf{x}_i$ = a vector of baseline characteristics;

A_i = an age "counter" for two-year age groups, beginning at ages 28-29 ($A_i = 1$) and continuing to ages 16-17 ($A_i = 7$);

K_i = 1 if age 16-17 at random assignment, 0 otherwise;

L_i = 1 if age 18-19 at random assignment, 0 otherwise;

M_i = 1 if age 20-21 at random assignment, 0 otherwise;

and

T_i = 1 for treatment group members, 0 for control group members.

The expression in parentheses models impact as a linear function of the age counter and the dummy variables for the youth age groups. If impacts on the earnings of youths follow the impact-versus-age trend established by impacts on the earnings of adults, then $\alpha = \beta = \gamma = 0$.

The vector $\mathbf{x}_i$ used consists of all regressors listed in the youth column of Exhibit D.1 except the age dummies. We estimate the model by regressing earnings on a constant, $\mathbf{x}_i$, A_i, K_i, L_i, M_i, T_i, and the interactions of T_i with A_i, K_i, L_i, and M_i. We then perform t tests of the three null hypotheses $\alpha = 0$; $\beta = 0$; and $\gamma = 0$. The estimates of α, β, and γ are reported in Exhibit H.22.

Exhibit D.1 *Baseline Characteristics Used as Regressors in Adjusting for Differences Between the Treatment Group and Control Group, by Target Group*

Regressor	*Adult Women*	*Adult Men*	*Out-of-School Youths*[a]
Ethnicity			
(White, non-Hispanic)			
Black, non-Hispanic			
Hispanic			
Asian, Pacific Islander, American Indian, or Alaskan Native			
Education and training histories			
Adult Basic Education or ESL[b]			
High school diploma			
GED certificate			
Some college			
Occupational training			
Technical certificate			
Job search assistance program			
Work histories			
Ever employed			
Employed upon application			
Total earnings in past 12 months[c]			
Hourly earnings in most recent job[c, d]			
Weeks worked in past 12 months			
(Zero)			
1-26 weeks			
27-52 weeks			
0-27 weeks			
(28-52 weeks)			
UI-reported earnings in each of 5 quarters before random assignment[c, e]			
Public assistance histories			
Receiving Food Stamps			
Receiving cash welfare other than AFDC[f]			
Receiving unemployment benefits			
Any unemployment benefits, past 12 months			

(continued)

Exhibit D.1

Baseline Characteristics Used as Regressors in Adjusting for Differences Between the Treatment Group and Control Group, by Target Group (Continued)

	Adult Women	*Adult Men*	*Out-of-School Youths*
AFDC histories			
Ever AFDC case head			
Case head anytime in past 12 months			
Received AFDC all of past 12 months			
Years as AFDC case head:			
(Less than 2 years)			
2-5 years			
More than 5 years			
JTPA required			
Required to apply[g]			
Household composition			
Marital status:			
(Never married)			
Spouse present			
Widowed, divorced, or separated			
Number of persons in household[h]			
Number of own children present[h]			
Own child under age 6 present			
Family income in past 12 months			
Less than $3,000			
$3,000-$6,000			
(More than $6,000)			
Living in public housing			
Yes			
Transportation / communication			
Driver's license			
Car available for regular use			
Telephone at home			
Age at random assignment[i]			
(16-19)			
20-21			
22-29			
30-44			
45-54			
(55 or older)			

(continued)

Exhibit D.1 *Baseline Characteristics Used as Regressors in Adjusting for Differences Between the Treatment Group and Control Group, by Target Group (Continued)*

	Adult Women	*Adult Men*	*Out-of-School Youths*
Recommended program services			
6 dummy variables, not mutually exclusive[j]	☐	☐	☐
Site			
15 mutually exclusive dummy variables[k] (1 dummy variable omitted)	☐	☐	☐
14 mutually exclusive dummy variables[k] (2 dummy variables omitted)	☐	☐	☐
Sampling cohort			
Months between date site began random assignment and date individual assigned:[l]			
(Less than 7 months)			
7-12 months	☐	☐	☐
More than 12 months	☐	☐	☐

Key:
☐ Used as a regressor
☐ Not used as a regressor

Notes: Each regressor is a dummy variable, except where noted otherwise. Variables in parentheses are omitted from the regressions to avoid exact collinearity.

Source: Background Information Form responses, except where noted otherwise.

a. The same set of regressors is used for both female and male out-of-school youths. When pooling females and males, we add a dummy variable for gender.

b. English as a Second Language.

c. A continuous (not a dummy) variable.

d. Set equal to a constant if never employed. (Because a dummy variable for "ever employed" is also included, the particular constant chosen is irrelevant to the impact estimates.)

e. Source: Earnings data from state Unemployment Insurance (UI) agencies.

f. General Assistance, Home Relief, or any other welfare that is not AFDC or Food Stamps.

g. As a requirement to receive welfare, Food Stamps, or as part of the federal Work Incentive (WIN) program; or by a court order.

h. A count (not a dummy) variable.

i. Sources: Background Information Form responses (for date of birth) and random assignment telephone file (for date of random assignment).

j. The six service categories are: classroom training in occupational skills; on-the-job training; job search assistance; basic education; work experience; and miscellaneous services.

k. Fifteen site dummy variables are used with the adult target groups; 14 are used with the youth target groups, which have no 18-month study sample members in Oakland.

l. Source: Random assignment telephone file.

Exhibit D.2 Derivation of Estimated Percentage Impacts on the Number of Weeks and Hours Worked, Adjusted for Survey Nonresponse Bias

	Impact as a percentage of control group mean	
Outcome measure (by quarter or over all quarters)	Estimate before adjustment (based on respondents)	Estimate adjusted for nonresponse bias
Percentage employed at any time during period	a	x
Weeks worked during period	b	$x + (b-a)[(z-x)/(d-a)]$
Hours worked during period	c	$x + (c-a)[(z-x)/(d-a)]$
Earnings during period	d	z

Exhibit D.3 *Estimated Impacts on Earnings, with and without Nonrespondent Imputations: Adult Women*

Period	Control mean (1)	Impact per assignee, in $ (2)
	Respondents only	
Quarter 1	$ 911	$ 37
2	1,139	79**
3	1,235	140***
4	1,376	92**
5	1,427	132***
6	1,425	160***
All quarters	7,507	645***
	With nonrespondent imputations	
Quarter 1	$ 916	$ 26
2	1,145	60*
3	1,236	118***
4	1,363	78**
5	1,413	116***
6	1,414	141***
All quarters	7,488	539***

Source: Top panel, estimates based on First Follow-up Survey responses; bottom panel, estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, top panel, assignees = 3,881; control group = 1,844. Bottom panel, assignees = 4,376; control group = 2,098.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Exhibit D.4 Estimated Impacts on Employment, with and without Nonrespondent Imputations: Adult Women

Period	Percentage employed		Weeks worked		Hours worked	
	Control mean (1)	*Impact in % (2)*	*Control mean (3)*	*Impact in weeks (4)*	*Control mean (5)*	*Impact in hours (6)*
			Respondents only			
Quarter 1	47.9%	0.4%	5.0	0.0	181	0
2	53.2	1.6	6.0	0.1	220	4
3	56.1	2.4*	6.3	0.3*	233	14**
4	60.1	1.0	6.8	0.1	252	9
5	59.5	3.9***	7.0	0.3**	258	15**
6	61.0	3.3**	7.0	0.5***	254	20***
All quarters	76.4	2.6**	38.1	1.3*	1,397	62*
			With nonrespondent imputations			
Quarter 1	48.4%	0.0%	5.1	0.0	183	-1
2	53.4	1.4	6.0	0.1	220	3
3	55.8	2.4*	6.3	0.3*	232	12*
4	59.7	0.8	6.8	0.1	250	8
5	59.3	3.3***	7.0	0.3	257	12*
6	60.9	2.2*	6.9	0.3**	254	17**
All quarters	76.8	2.1**	38.3	1.1	1,403	52

Source: Top panel, estimates based on First Follow-up Survey responses; bottom panel, estimates based on First First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Sample size, top panel, assignees = 3,881, control group = 1,844. Bottom panel, assignees = 4,376; control group = 2,098. * Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Appendix E

A Comparison of Earnings, Employment, and Impact Estimates Based on Data from the First Follow-Up Survey and from State Unemployment Insurance Agencies

THIS appendix compares follow-up earnings and employment estimates, as well as impact estimates, from two data sources: First Follow-up Survey responses, and earnings data from state unemployment insurance (UI) agencies.[1] The First Follow-up Survey is the main source of impact data for the present report, and UI earnings records will be a major source of impact data for our final report. Hence, the comparability of the earnings and employment information from these two sources is an issue of central concern for the National JTPA Study. This appendix should also be of more general interest to researchers who must choose between the two types of data sources in future evaluations of employment and training programs.

The findings in this appendix are for a special sample constructed solely to compare the two data sources. This *12-month comparison sample* includes all treatment group and control group members in the 18-month study sample for whom earnings data were available from both data sources for the first four calendar quarters after random assignment. Because UI earnings data are not currently available from New Jersey and

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[1] See Appendix C for a description of these two data sources.

Ohio, sample members from the two study sites in those states—Jersey City and Marion—are not included in the analysis here.

We limited the analysis to the first four calendar quarters after each sample member's random assignment, because it was that period for which the most survey and UI data were available and because UI records report earnings only by calendar quarter. The sample is therefore constant across all four follow-up quarters. Because this comparison sample is a subsample of the 18-month study sample, none of the survey-based estimates presented below match those elsewhere in the report.

Our comparison focuses on the only two outcome measures currently available from the UI data set: quarterly earnings, and quarterly employment.[2] The *survey-based earnings measure* is defined as total quarterly earnings from wages, salaries, tips, and bonuses.[3] The *UI-based earnings measure* is defined as all types of earnings from all jobs reported to the state's unemployment insurance system. For both data sets *employed individuals* are defined as those with positive earnings in the period.

Several factors may cause the earnings and employment estimates from the two sources to differ. First, although UI earnings records cover over 90 percent of all workers, they do not report on the earnings of persons who are self-employed or who are railroad, federal, or out-of-state employees. Second, UI records may miss unreported earnings from casual work, the underground economy, or tips from reported jobs. Third, they may report severance pay as regular earnings. And finally, UI records may contain random reporting errors and incorrect social security numbers.

On the other hand, the survey-based estimates may contain other errors stemming from recall problems. Some survey respondents may have forgotten to report a particular job or may have inflated their earnings in the interview, although they had no obvious incentive to do so. Random reporting or coding errors could also occur in the survey data.

Within any quarter we observe, the earnings estimates from the two data sources may also conflict because of timing differences. The survey respondents, for example, may have misstated the start date or end date of a job spell whereas employers, particularly small businesses, may have reported earnings to UI agencies with a lag, making the UI records for a given quarter incomplete. Moreover, UI records report earnings when they are paid, not earned, whereas survey respondents may not have followed this convention.

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[2] An extension of this analysis in our final report will incorporate a variable indicating the number of jobs reported each quarter.

[3] The survey-based measure of quarterly earnings excludes earnings from so-called odd jobs reported in the First Follow-up Survey. *Odd-job earnings* during the follow-up period were not part of a specific job spell.

Thus, the possibilities of differences in reporting errors from the two data sources are numerous, leading to a potential for differences in the earnings and employment estimates calculated from each source. If the reporting differences are random, they will not bias the estimates of program impacts. But if the reporting differences are systematic, and especially if they differ between the treatment group and the control group, the two data sources will yield different impact estimates.

The analysis described below does in fact find consistent differences between the earnings and employment estimates based on First Follow-up Survey responses and those based on UI earnings data, but it was not possible to identify fully the reasons for those differences. An expanded version of this analysis to be presented in our final report will explore the reasons in more detail.

For now, however, our principal findings are twofold:

- The survey-based estimates of average earnings are consistently and substantially higher than the UI-based estimates.

- Nevertheless, the estimated JTPA impacts on earnings are similar for the two sets of estimates, in the cases of adult women, adult men, female out-of-school youths, and most male out-of-school youths. The main exception to this rule was the subgroup of male youths who had been arrested between their sixteenth birthday and when they applied to JTPA (25 percent of all male youths in the treatment group). For this subgroup the survey data indicated a large negative impact, whereas UI data indicated a negligible impact.

The remainder of this appendix details the findings from the analyses.

Differences in Earnings Estimates

This section reports the results of four separate analyses of differences in the earnings estimates based on the survey and UI data. The first analysis, of differences in the earnings distributions estimated from each data source, establishes the pattern of relatively higher survey-based than UI-based earnings estimates. The other three analyses investigate the possibility of site- or time-specific reasons for this pattern.

DISTRIBUTIONS OF 12-MONTH EARNINGS

A simple way to compare earnings estimates based on the two data sources is to compare total reported earnings for all four quarters combined. Our comparison therefore begins with frequency distributions of the mean earnings of the 12-month comparison sample (including the treatment and control groups) over the first year after random assignment. As shown in Exhibit E.1, the UI data reported more sample members earning under $10,000 than did the survey. Correspondingly, the survey data show more sample members in the higher earnings categories up to $40,000 annually. These findings are consistent across all four target groups.

The tendency for the earnings distribution based on the survey data to lie above the earnings distribution based on the UI data is offset somewhat by a slightly larger proportion of sample members with zero 12-month earnings in the survey data than in the UI data.

12-MONTH EARNINGS BY SITE

In a second analysis we examined whether the finding of relatively higher survey-based earnings estimates holds true across the 16 study sites. If the survey-based estimates were higher than the UI-based estimates in only a few of the sites, we would then need to explore site-specific reasons for the discrepancies.

Exhibit E.2 presents a survey-UI comparison of mean annual earnings, by site, for each target group. The ratio of the survey-based estimate to the UI-based estimate for each target group, by site, (column 3) serves as a simple summary statistic with which to compare discrepancies across the sites for which we have data. The findings are clear. The survey-based averages exceed the UI-based averages by a factor ranging from 35 percent for adult women overall to 80 percent for male out-of-school youths. This pattern is consistent across sites within each target group.[4]

This finding of consistently higher survey-based estimates across the sites rules out problems arising from data collection errors in only a few specific sites—or types of jobs found predominantly in only a few sites—as the source of the discrepancies. We therefore must consider more general explanations, such as erroneous reporting of job spells or earnings by the survey respondents or the widespread occurrence of jobs or types of earnings that tend to be missed in UI earnings reporting.

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[4] In each target group panel in the exhibit, the magnitude of the outlier ratio displayed in the first row (for instance, 4.66 in the case of adult women) prompted further exploration, which indicated that the UI earnings data for that site were incomplete. Revised data on the site are not yet available.

12-Month Earnings, by Location Near a State Border

The consistency of the pattern across sites also suggests that out-of-state jobs held by sample members are not a major source of the difference between the survey- and UI based estimates. If sample members received a substantial amount of their earnings from employers outside the state in which they lived when they applied to JTPA (for example, because they commuted to out-of-state jobs or subsequently moved to a neighboring state), one would expect the survey-based earnings estimates to exceed the UI-based estimates by substantially more in those sites that were near a state border.

But as shown in Exhibit E.3, the ratio of survey to UI-based earnings estimates differed little between sites near a state border and sites not near a border. Indeed, this ratio was slightly smaller, not larger, in those sites near a state border.[5]

Quarterly Earnings

The next step in our analysis was to determine whether the finding of relatively higher survey-based than UI-based earnings estimates was constant over all four quarters after random assignment. One might expect, for example, that the incidence of survey recall errors would decrease in more recent quarters, whereas lagged reporting by employers might cause earnings to be under-reported to a greater extent in more recent quarters. Moreover, as time went on, sample members may have been more or less likely to find the types of jobs that would be reported in one but not the other data source, such as federal jobs or self-employed work.

Exhibit E.4 displays estimates of mean earnings in each follow-up quarter, based on each of the two data sources. Again, the survey-UI ratio of these means is a convenient summary statistic to examine. The clear message of Exhibit E.4 is that the pattern of relatively higher survey-based estimates is stable across all of the first four quarters after random assignment.

The exhibit also shows that the discrepancies between the two data sources are statistically significant at the .01 level.[6] We therefore need an explanation of the relatively higher survey-based estimates that is relevant for *all* follow-up quarters.

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[5] A site was classified as near a state border if it included any counties adjacent to another state.

[6] Although Exhibit E.4 shows the ratio of the two earnings estimates, we tested whether this ratio was significantly different from 1 (one) by employing a two-tailed t-test of the hypothesis that the paired *differences* between the survey and UI-based earnings was zero.

Differences in Estimated Employment Rates versus Differences in Estimated Earnings per Employed Sample Member

To look beyond simple comparisons of total earnings, we decomposed the survey-UI ratio of total earnings into the product of two components: the survey-UI ratio of *employment rates*, and the survey-UI ratio of mean *earnings per employed sample member*. If most of the survey-UI difference in total earnings arose from differences in the quarterly employment rates reported, any explanation of the larger survey-based estimates of total earnings should stress factors such as jobs that the survey but not the UI records reported, delayed reporting of jobs in the UI data, or exaggerated lengths of job spells in the survey data.

On the other hand, if most of the survey-UI difference in total earnings arose from differences in reported earnings per employee, we should suspect such reasons for the discrepancy as differences in the types of jobs each data source reported, tip income or second jobs reported only by the survey data, or exaggeration of income or job spell lengths in the survey data.

EMPLOYMENT RATES

As the first step in this analysis Exhibit E.5 presents quarterly and 12-month employment rates obtained from the two data sources, by target group. The *percentage employed* is defined as the percentage of the sample with any recorded earnings during the period in question. The exhibit indicates that the employment rates based on the two data sources are not nearly as different as the earnings estimates were shown to be in the preceding section. For each target group the survey-based employment rates were actually slightly lower in the first quarter and for all four quarters overall than the UI-based employment rates. But in each target group the survey-UI ratio increased over the last three quarters. In the fourth quarter the survey-based employment rate exceeded the UI-based rate by 7 percent to 10 percent for females (column 3) and by 16 percent to 19 percent for males in the sample (column 6).[7]

The main lesson of Exhibit E.5 is therefore that the relatively higher survey-based than UI-based estimates of total earnings are not largely attributable to higher survey-based employment rates.

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[7] The ratio of survey-based to UI-based employment rates (or mean earnings) provides a direct indication of the percentage by which the survey-based estimate exceeds or falls short of the UI-based estimate. For example, if the ratio is 1.07, the survey-based estimate is 7 percent *larger* than the UI-based estimate; and if the ratio is 0.93, the survey-based estimate is 7 percent *smaller* than the UI-based estimate.

Nevertheless, we still need an explanation for the fourth quarter findings. One possible explanation for the higher survey-based rates in the fourth quarter is employer delays in reporting earnings to state UI agencies. If some employers report the earnings of their employees to the state later than others, then at any given time the state UI records that are most incomplete are those for the most recent quarter. Thus, the UI earnings data we obtained from the states would be less complete for the most recent quarters. This would yield a pattern in which the UI-based estimates of earnings and employment understated true earnings and employment to a greater extent in the later follow-up quarters.

One way to explore this possibility is to examine the UI-based employment rates of the subsample of the 12-month comparison sample with six quarters of complete UI earnings data. If reporting delays were a problem, one would expect to see higher reported employment rates during the first four follow-up quarters for the six-quarter subsample than for the 12-month comparison sample. But as shown in Exhibit E.6 there is no consistent difference between the UI-based employment rates for the six-quarter subsample and those for the full comparison sample. Thus, there is no evidence of reporting delays in the UI earnings data used in the present analysis.

QUARTERLY EARNINGS PER EMPLOYED SAMPLE MEMBER

Since the survey-UI discrepancies in employment rates are fairly small, we must look to the second component of total earnings for some insight into the issue. Exhibit E.7 displays the mean earnings of employed sample members, by target group, during each quarter and over all four quarters. The results here confirm that most of the difference between the survey and UI-based estimates of total earnings reflects the fact that the survey reported higher earnings per employed sample member. Moreover, the survey-UI ratios for earnings per employee over all four quarters are very close to the ratios for total 12-month earnings, shown in Exhibit E.4. Nevertheless, the survey-UI ratios of earnings per employee in Exhibit E.7 fall steadily from the first to the fourth quarter after random assignment, for all four target groups.

Indeed, when we examine Exhibits E.4 *through* E.7 together, an interesting time pattern emerges. The constancy of survey-UI ratios of total earnings over time is the result of two countervailing trends: First, the survey-UI ratios of employment rates rise over time, and second, the survey-UI ratios of earnings per employee fall over time.

We can only speculate about the forces underlying these trends. For example, survey-UI ratios of employment rates would rise over time if survey respondents were less likely to forget more recent jobs. An explanation of the falling survey-UI ratios of earnings per employee is harder to come by, however. Perhaps respondents were more likely to exaggerate earnings or job spell lengths when recalling jobs in the more distant past.

Further Analysis of Differences in Earnings per Employed Sample Member

To learn more about the survey-UI discrepancies in estimated earnings per employed sample member, we decomposed the survey-UI ratio of earnings per employee into two parts: the survey-UI ratio of earnings per employee for the subsample of individuals who were *reported as employed in both data sets;* and the survey-UI ratio of earnings per employee for the subsample of individuals who were *reported as employed in only one of the data sets.*

The first step in this analysis was to examine the distribution of observations in which reported employment was consistent or inconsistent between the two data sources. To do so, we created a "pooled" analysis sample in which each follow-up quarter for each member of the 12-month comparison sample was a separate observation. Thus, each sample member was represented by four observations (for quarters 1, 2, 3, and 4) in the pooled analysis sample. We refer to each of these observations as a *person-quarter.*

Exhibit E.8 presents the percentage of these person-quarters for which neither data source or both data sources reported some earnings (and hence were consistent) and the percentage of these person-quarters for which either the First Follow-up Survey only or UI earnings data only reported some earnings (and hence the two sources were inconsistent). As shown in the last row of the exhibit, the two data sources report employment status consistently for 67 percent to 78 percent of the person-quarters in the pooled analysis sample. The remaining 22 percent to 33 percent of the person-quarters are divided roughly evenly between observations with employment reported only by the survey data and observations with employment reported only by the UI data. This pattern persists even if we look at each of the first, second, third, and fourth follow-up quarters separately (not shown in the exhibit).

We then compared mean earnings per employee for person-quarters with employment reported in both data sources and for person-quarters with employment reported in only one data source. Exhibit E.9 presents our findings, which indicate that even when we compare earnings per employee for the subsamples with employment reported in both data sets, the survey-based estimates exceed the UI-based estimates by a factor ranging from 24 percent in the case of adult women to 56 percent in the case of male youths. The sources of these relatively higher survey-based estimates of earnings per employee might be second jobs or tips reported only in the survey data or over-reporting of earnings or job spell lengths in the survey data.

As shown in the second panel of Exhibit E.9, mean earnings per employee for person-quarters with employment reported only in the survey are more than twice as large as those for person-quarters with employment reported only in the UI data. Moreover, the estimated earnings for person-quarters with employment reported only in the survey (and therefore "missed" by the UI data) are, on average, of nearly the same magnitude as the survey-based estimates for person-quarters with employment reported in both data sources (top panel).[8] In other words, the UI data may miss some quarters in which sample members had fairly typical earnings. A plausible explanation for this pattern is that UI data miss fairly typical quarters because of random errors in, say, reported social security numbers.

Exhibit E.9 also indicates that estimated earnings from employment reported only in the UI data (and thus missed in the survey data) are, on average, about half as large as the UI-based estimates of earnings from employment reported by both data sources. The survey data may therefore miss low-wage jobs, or person-quarters during which sample members were employed only briefly. This pattern may arise if survey respondents tended to forget short-term, low-wage jobs. It may also reflect misreporting by survey respondents of the start and end dates of their job spells, such that portions of quarters in which the respondents were actually employed went unrecorded.

Differences in Estimated Program Impacts on Earnings and Employment

Given the substantial differences in earnings estimates from the two data sources, a crucial question is whether these differences translate into different program impact estimates. As noted earlier, the answer depends on whether the survey-UI discrepancies differ substantially between the treatment group and the control group.

Fortunately, this does not appear to be the case for any of the target groups. The ratio of mean survey earnings to mean UI earnings during the first four follow-up quarters was 1.35 for adult women in the treatment group and 1.35 for adult women in the control group. For adult men the rations were 1.53 and 1.52, respectively; for female youths they were 1.47 and 1.48; and for male youths, 1.79 and 1.83. Hence, neither data source appreciably over- or under-reported the average earnings of the treatment group or the control group relative to the other, although there was a slight reporting difference for male youths.

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[8] We must qualify the notion that the UI data "miss" quarters with earnings, since "employment" reported only in the survey may arise from erroneous reporting of job spells in the survey.

Exhibit E.10 presents estimates of program impacts on quarterly and 12-month earnings, by target group, using both survey data and UI data. For all four target groups the differences between the survey-based impact estimates and the UI-based estimates are not statistically significant (columns 3 and 6).[9]

Most of the difference between the impact estimates from the two data sources reflects a "scaling" of the result due to the fact that the survey reports more earnings than UI wage records for both the treatment group and the control group. For example, if the survey-UI earnings ratio were 1.5 for both the treatment and the control group the impact estimate from the survey data would be 1.5 times the corresponding estimate from the UI data.

To eliminate this scaling effect when comparing the impact estimates from the two data sources, one can express the estimated impacts as a percentage of their respective mean control group earnings. Doing so for the estimates of impact on total earnings during the four follow-up quarters in Exhibit E.10 yields an 11.4 percent survey-based impact estimate and a 10.9 percent UI-based impact estimate for adult women. Corresponding estimates are 4.1 percent and 3.5 percent for adult men; less than 0.1 percent and less than 0.1 percent for female youths; and -8.2 and -6.5 percent for male youths. Hence, the impact findings from the two data sources tell virtually the same story for each target group, although they differ most noticeably for male youths.[10]

A key exception to this rule is the impact estimate for male youths with a previous arrest, the small minority of male youths who accounted for most of the negative average impact estimate for male youths overall (see Chapter 6). For male youths with a previous arrest, the survey-UI earnings ratio was 1.89 for treatment group members and 2.62 for control group members. For this group, the survey data yielded a significant $-1,777 estimated earnings loss during the first four follow-up quarters, whereas UI data yielded an insignificant $89 estimated earnings gain.

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[9] We tested whether the survey-based and UI-based estimates of impacts on earnings were significantly different by running a regression of the *difference* between survey-reported and UI-reported earnings on an indicator variable set equal to one for treatment group members and zero for control group members. The two impact estimates were significantly different if the coefficient of the treatment indicator was significantly different from zero.

[10] Slight differences between the survey-UI earnings ratios for the treatment and control groups can generate a much larger proportional difference between the impact estimates from the two data sources. Consider the following example: (1) mean UI-based earnings are $5,500 for the treatment group and $5,000 for the control group and (2) mean survey-based earnings are 1.70 times mean UI-based earnings for the treatment group and 1.65 times mean UI-based earnings for the control group. The estimated program impact is thus $500 according to the UI data and $1,100 according to the survey data. The survey-based impact estimate is therefore 2.2 times the UI-based impact estimate.

Preliminary analyses of the types of jobs reported on the survey (mostly low-wage service jobs) do not provide any reason to doubt the validity of the survey data. However, until we can fully explain the discrepancy between the impact estimates based on the survey data and those based on the UI data, one should interpret with caution the survey-based impact estimates for male youths with a previous arrest presented in this report. In our final report we will explore this issue in much greater detail.

Program impact estimates from the two data sources were much more consistent with each other for male youths with no previous arrest. They were $-185 from the survey data and $-277 from the UI data. Therefore, for most of the study sample—adult women, adult men, female youths, and male youths with no previous arrests—the two data sources produced similar results.

Summary

The preceding findings indicate that average earnings of the 12-month comparison sample are substantially higher when estimated from First Follow-up Survey data than when estimated from UI earnings data. This differential was consistently observed for all four target groups in the study, for all 14 sites included in this analysis, and for all four calendar quarters after random assignment. Hence, the problem does not appear to reflect idiosyncratic, localized issues but rather one or more pervasive differences between how the two data sources measure earnings.

With the information currently available we have been able to explore the reasons for the survey-UI difference in earnings estimates to a limited extent. Our final report will present a more detailed analysis. Nevertheless, from the present analysis it appears that:

- Almost all of the survey-UI difference in average earnings is due to higher survey-based estimates of earnings per employed sample member; very little of the difference is due to a difference in estimated employment rates between the two data sources.

- Much of the survey-UI difference in average earnings is due to higher survey-based estimates of earnings for those sample members who were reported as employed by both data sources.

- The remainder of the difference reflects the possibility that the survey tended to miss low-paying jobs, or jobs that sample members held only briefly, whereas the UI wage records tended to miss jobs with roughly average earnings per quarter.

- The extent to which differences in the earnings measures from the two data sources were translated into different estimates of program impacts was limited. Estimates of program impacts on earnings were not statistically significantly different between the two data sources for any of the four target groups. For one target group, however—male out-of-school youths—the point estimates of program impacts were noticeably different. However, most of this difference was concentrated within the small minority of male youths with a previous arrest. Therefore on balance, the impact findings from the two data sources were consistent for adult women, adult men, female youths, and most male youths.

Given the central role played by survey- and UI-based earnings data in the National JTPA Study, and the considerable extent to which other researchers rely on both types of data sources, the analysis in this appendix must be viewed as a first step in the crucial task of exploring the nature and causes of the differences in the information obtained from the two data sources.

Exhibit E.1 *Distributions of Survey- and UI-Based Estimates of Mean 12-Month Earnings: The 12-Month Comparison Sample, by Target Group*

12-month earnings	*First Follow-up Survey (1)*	*UI earnings data (2)*	*Difference (1) - (2) (3)*	*First Follow-up Survey (4)*	*UI earnings data (5)*	*Difference (4) - (5) (6)*
		Adult women			*Adult men*	
$0	1,222	1,136	86	623	608	15
$1 - $10,000	2,527	3,015	-488	1,528	2,152	-624
$10,001 - $20,000	834	492	342	1,093	679	414
$20,001 - $30,000	72	21	51	214	80	134
$30,001 - $40,000	14	4	10	57	8	49
> $40,000	0	1	-1	14	2	12
Sample size	*4,669*	*4,669*		*3,529*	*3,529*	
		Female youths			*Male youths*	
$0	587	484	103	274	241	33
$1 - $10,000	1,160	1,368	-208	804	1,093	-289
$10,001 - $20,000	179	84	95	321	107	214
$20,001 - $30,000	12	2	10	40	4	36
$30,001 - $40,000	1	1	0	8	1	7
> $40,000	0	0	0	0	1	-1
Sample size	*1,939*	*1,939*		*1,447*	*1,447*	

Sources: Estimates based on First Follow-up Survey responses and earnings data from state Unemployment Insurance (UI) agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. Tests of statistical significance were not performed for this exhibit.

Exhibit E.2 *Survey- and UI-Based Estimates of Mean 12-Month Earnings: The 12-Month Comparison Sample, by Target Group and Study Site*

Study site, by size of the ratio	*First Follow-up Survey (1)*	*UI earnings data (2)*	*Ratio (1) / (2) (3)*	*First Follow-up Survey (4)*	*UI earnings data (5)*	*Ratio (4) / (5) (6)*
		Adult women			*Adult men*	
1	$ 4,974	$ 1,067	4.66[a]	$ 10,615	$ 3,971	2.67[a]
2	5,437	3,504	1.55	6,592	3,386	1.95
3	4,606	3,154	1.46	9,287	5,588	1.66
4	5,314	3,771	1.41	7,614	4,591	1.66
5	4,905	3,527	1.39	9,355	5,716	1.64
6	6,335	4,595	1.38	9,626	6,162	1.56
7	4,329	3,204	1.35	7,415	4,775	1.55
8	4,652	3,482	1.34	9,189	6,237	1.47
9	5,527	4,262	1.30	8,704	5,975	1.46
10	4,605	3,593	1.28	6,890	4,788	1.44
11	5,510	4,307	1.28	8,192	5,848	1.40
12	4,912	3,862	1.27	7,652	5,468	1.40
13	6,782	5,402	1.26	9,449	6,756	1.40
14	4,621	4,447	1.04	9,705	7,085	1.37
All sites	*5,334*	*3,944*	*1.35*	*8,766*	*5,740*	*1.53*
Sample size	*4,669*	*4,669*		*3,529*	*3,529*	
		Female youths			*Male youths*	
1	$ 4,175	$ 859	4.86[a]	$ 5,039	$ 1,109	4.54[a]
2	4,553	2,624	1.74	10,552	3,982	2.65
3	4,015	2,391	1.68	6,529	2,741	2.38
4	4,088	2,641	1.55	4,128	2,091	1.97
5	2,476	1,668	1.48	8,923	4,520	1.97
6	2,418	1,634	1.48	5,319	2,777	1.92
7	4,852	3,308	1.47	6,361	3,479	1.83
8	3,912	2,687	1.46	6,974	3,882	1.80
9	3,787	2,778	1.36	5,012	2,873	1.74
10	5,620	4,228	1.33	7,587	4,394	1.73
11	3,660	2,835	1.29	8,662	5,178	1.67
12	4,889	3,926	1.25	5,476	3,369	1.63
All sites	*3,819*	*2,594*	*1.47*	*6,396*	*3,545*	*1.80*
Sample size	*1,939*	*1,939*		*1,447*	*1,447*	

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. Tests of statistical significance were not performed for this exhibit. Results are not reported for sites with fewer than 10 sample members.

a. UI earnings data for this site are currently incomplete.

Exhibit E.3 *Survey- and UI-Based Estimates of Mean 12-Month Earnings in Sites Located Near a State Border and in Those Not: The 12-Month Comparison Sample, by Target Group*

	Adult women (1)	*Adult men (2)*	*Female youths (3)*	*Male youths (4)*
Sites near a state border [a]				
First Follow-up Survey	$ 5,715	$ 8,898	$ 4,387	$ 7,110
UI earnings data	$ 4,352	$ 5,995	$ 3,003	$ 4,049
Survey-UI ratio	1.31	1.48	1.46	1.76
Sample size	*2,989*	*2,226*	*1,207*	*929*
Sites not near a state border [b]				
First Follow-up Survey	$ 4,655	$ 8,540	$ 2,884	$ 5,115
UI earnings data	$ 3,217	$ 5,303	$ 1,918	$ 2,641
Survey-UI ratio	1.45	1.61	1.50	1.94
Sample size	*1,680*	*1,303*	*732*	*518*

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. Tests of statistical significance were not performed for this exhibit.

[a] This category includes the following sites: Fort Wayne, Ind.; Coosa Valley, Ga.; Providence, R.I.; Springfield, Mo.; Omaha, Neb.; Larimer County, Colo. and Northwest Minnesota.

[b] This category includes Corpus Christi, Tex.; Jackson, Miss.; Oakland, Calif.; Heartland, Fla.; Butte, Mont.; Decatur, Ill. and Cedar Rapids, Iowa.

Exhibit E.4 *Survey- and UI-Based Estimates of Mean Quarterly and 12-Month Earnings: The 12-Month Comparison Sample, by Target Group*

Period	*First Follow-up Survey (1)*	*UI earnings data (2)*	*Ratio (1) / (2) (3)*	*First Follow-up Survey (4)*	*UI earnings data (5)*	*Ratio (4) / (5) (6)*
		Adult women			*Adult men*	
Quarter 1	$ 1,090	$ 789	1.38***	$ 1,908	$ 1,236	1.54***
2	1,294	954	1.36***	2,169	1,432	1.51***
3	1,424	1,075	1.32***	2,311	1,527	1.51***
4	1,526	1,126	1.36***	2,378	1,545	1.54***
All quarters	5,334	3,944	1.35***	8,766	5,740	1.53***
Sample size	*4,669*	*4,669*		*3,529*	*3,529*	
		Female youths			*Male youths*	
Quarter 1	$ 801	$ 536	1.49***	$ 1,365	$ 794	1.72***
2	935	618	1.51***	1,583	878	1.80***
3	1,028	708	1.45***	1,678	917	1.83***
4	1,056	732	1.44***	1,771	957	1.85***
All quarters	3,819	2,594	1.47***	6,396	3,545	1.80***
Sample size	*1,939*	*1,939*		*1,447*	*1,447*	

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report.

* Statistically significantly different from one at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Exhibit E.5 *Survey- and UI-Based Estimates of the Percentage Employed: The 12-Month Comparison Sample, by Target Group*

Period		*First Follow-up Survey (1)*	*UI earnings data (2)*	*Ratio (1) / (2) (3)*	*First Follow-up Survey (4)*	*UI earnings data (5)*	*Ratio (4) / (5) (6)*
			Adult women			*Adult men*	
Quarter	1	52.7%	55.4%	0.95***	64.9%	65.6%	0.99
	2	57.6	57.1	1.01	69.2	65.0	1.07***
	3	60.8	57.7	1.05***	70.9	63.7	1.11***
	4	63.2	57.5	1.10***	71.9	60.6	1.19***
All quarters		73.8	76.0	0.97***	82.3	82.8	0.99
Sample size		*4,669*	*4,669*		*3,529*	*3,529*	
			Female youths			*Male youths*	
Quarter	1	46.8%	51.0%	0.92***	58.5%	61.6%	0.95**
	2	50.6	52.7	0.96*	64.9	61.7	1.05**
	3	52.0	53.6	0.97*	65.0	61.4	1.06***
	4	55.8	52.1	1.07***	70.0	60.3	1.16***
All quarters		70.0	75.0	0.93***	81.0	83.3	0.97*
Sample size		*1,939*	*1,939*		*1,447*	*1,447*	

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. The "percentage employed" is the percentage of the sample with any recorded earnings during the period.

* Statistically significantly different from one at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Exhibit E.6 *UI-Based Estimates of the Percentage Employed: The 12-Month Comparison Sample and the Subsample with Six Quarters of UI Earnings Data, by Target Group*

Period	*Full 12-month sample (1)*	*6-quarter subsample (2)*	*Full 12-month sample (3)*	*6-quarter subsample (4)*
	Adult women		*Adult men*	
Quarter 1	55.4%	56.5%	65.6%	66.5%
2	57.1	58.3	65.0	64.9
3	57.7	59.0	63.7	64.1
4	57.5	59.0	60.6	61.3
All quarters	76.0	77.2	82.8	84.2
Sample size	*4,669*	*3,831*	*3,529*	*2,922*
	Female youths		*Male youths*	
Quarter 1	51.0%	51.9%	61.6%	61.8%
2	52.7	52.6	61.7	61.9
3	53.6	54.2	61.4	61.7
4	52.1	52.7	60.3	61.2
All quarters	75.0	75.4	83.3	84.6
Sample size	*1,939*	*1,695*	*1,447*	*1,236*

Source: Estimates based on earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. The "percentage employed" is the percentage of the sample with any recorded earnings during the period.

Exhibit E.7 *Survey- and UI-Based Estimates of Mean Quarterly and 12-Month Earnings per Employed Sample Member: The 12-Month Comparison Sample, by Target Group*

Period	*First Follow-up Survey (1)*	*UI earnings data (2)*	*Ratio (1) / (2) (3)*	*First Follow-up Survey (4)*	*UI earnings data (5)*	*Ratio (4) / (5) (6)*
	Adult women			*Adult men*		
Quarter 1	$ 2,069	$ 1,424	1.45	$ 2,942	$ 1,886	1.56
2	2,245	1,673	1.34	3,136	2,211	1.42
3	2,342	1,862	1.26	3,259	2,397	1.36
4	2,413	1,957	1.23	3,307	2,550	1.30
All quarters	7,224	5,212	1.39	10,645	6,934	1.54
Sample size	*4,669*	*4,669*		*3,529*	*3,529*	
	Female youths			*Male youths*		
Quarter 1	$ 1,708	$ 1,052	1.62	$ 2,329	$ 1,289	1.81
2	1,844	1,174	1.57	2,440	1,422	1.72
3	1,991	1,320	1.51	2,574	1,492	1.73
4	1,897	1,404	1.35	2,524	1,587	1.59
All quarters	5,478	3,457	1.58	7,890	4,254	1.85
Sample size	*1,939*	*1,939*		*1,447*	*1,447*	

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. Tests of statistical significance were not performed for this exhibit. Mean earnings per employed sample member was computed by dividing mean earnings per sample member (Exhibit E.4) by the percentage of sample members who were employed (Exhibit E.5), and multiplying this result by 100. The "percentage employed" is the percentage with any recorded earnings during the period.

Exhibit E.8 ***Percentage of Person-Quarters in Which Employment Was Reported in the First Follow-up Survey Only, UI Earnings Data Only, Both Data Sources, or Neither Source: The 12-Month Comparison Sample, by Target Group***

Data source	*Adult women (1)*	*Adult men (2)*	*Female youths (3)*	*Male youths (4)*
Inconsistent reporting				
First Follow-up Survey only	12.0%	16.4%	11.9%	18.2%
UI earnings data only	10.3	10.8	13.0	14.8
One source or the other	22.4	27.2	24.9	33.0
Consistent reporting				
Neither source	31.1%	20.0%	35.8%	20.5%
Both sources	46.6	52.8	39.3	46.5
Neither or both sources	77.6	72.8	75.1	67.0

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.
Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. Tests of statistical significance were not performed for this exhibit. "Employment" is defined as any recorded earnings during the period.

Exhibit E.9 *Mean 12-Month Earnings for Person-Quarters with Employment Reported in One Data Source or Both Sources: The 12-Month Comparison Sample, by Target Group*

Data sources reporting employment	*Adult women (1)*	*Adult men (2)*	*Female youths (3)*	*Male youths (4)*
Both sources				
Survey-based estimate	$ 2,375	$ 3,216	$ 1,972	$ 2,527
UI-based estimate	1,915	2,462	1,434	1,620
Survey-UI ratio	1.24***	1.31***	1.38***	1.56***
Sample size	*8,699*	*7,456*	*3,050*	*2,691*
Only one source				
Survey-based estimate	$ 1,893	$ 3,008	$ 1,509	$ 2,330
UI-based estimate	907	1,240	649	903
Survey-UI ratio	2.09***	2.43***	2.33***	2.58***
Sample size, Survey	*2,244*	*2,314*	*922*	*1,054*
Sample size, UI	*1,932*	*1,529*	*1,009*	*855*

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report. "Employment" is defined as any recorded earnings during the period.

* Statistically significantly different from one at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Exhibit E.10 *Survey- and UI-Based Estimates of Program Impacts on Earnings: JTPA Assignees in the 12-Month Comparison Sample, by Target Group*

Period	*Survey-based impact (1)*	*UI-based impact (2)*	*Difference,* [a] *in $ (1) - (2) (3)*	*Survey-based impact (4)*	*UI-based impact (5)*	*Difference,* [a] *in $ (4) - (5) (6)*
		Adult women			*Adult men*	
Quarter 1	$ 91**	$ 40	$ 51	$ 17	$ 44	$ -27
2	163***	139***	24	156*	60	96
3	167***	85	82	112	78	34
4	145**	135***	10	68	12	56
All quarters	566***	399***	168	352	194	159
Sample size	*4,669*	*4,669*		*3,529*	*3,529*	
		Female youths			*Male youths*	
Quarter 1	$ -14	$ -43	$ 29	$ -196**	$ -83	$ -112
2	-6	-7	1	-28	-39	11
3	-47	11	-57	-182*	-50	-132
4	73	60	13	-149	-68	-81
All quarters	6	21	-15	-555*	-240	-315
Sample size	*1,939*	*1,939*		*1,447*	*1,447*	

Sources: Estimates based on First Follow-up Survey responses and earnings data from state UI agencies.

Notes: Usable UI earnings data for 2 of the 16 study sites (Jersey City, N.J., and Marion, Ohio) are not yet available, and so these sites are excluded from this analysis. Data on 4 out-of-school youths in Oakland, Calif., are included, although they were excluded from the 18-month analyses elsewhere in this report.

a. No difference in impacts is statistically significant at the .10 level (two-tailed test).

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Appendix F

Nonenrollees and an Analysis of Their Participation in JTPA

THIS appendix, which discusses the JTPA-related experience of the 36 percent of the treatment group that never enrolled in the program, serves two purposes. First, since nonenrollees did receive some JTPA services, it provides information on aspects of JTPA services received by the treatment group which would be missed by an exclusive reliance on enrollment data. Second, it provides needed context for interpreting the estimates of impact per enrollee presented in Chapters 4 through 7. Ideally, the report would present impacts per service recipient, but the only data available on the entire sample is JTPA enrollment. Information on JTPA services provided to nonenrolled assignees, therefore, provides information useful in assessing if impacts per enrollee differed in important ways from the ideal of impacts per service recipient.

The appendix begins by describing the JTPA enrollment process and the incentives within JTPA to delay enrollment. It then describes a special study using a small sample which examined the extent to which assignees who were not enrolled received any JTPA services.

Enrollment in JTPA Services

Enrollment in JTPA occurs when SDA staff enter applicants' names and application data into the local JTPA management information system and assign them to one or more specific JTPA-funded activities. This step makes each of these applicants an official JTPA participant, whose service receipt and progress is tracked and termination status (for example, employment and wages on leaving the program) is noted as part of the

JTPA performance standard system. By enrolling participants, SDA staff become accountable through the JTPA performance standard system for the costs JTPA incurs in serving them and their success when they leave the program.

In this study, random assignment occurred after local staff had determined applicants were eligible for JTPA and had assessed their interests, skills, and service needs and recommended appropriate JTPA services. Once an applicant was designated a member of the treatment group through random assignment, local staff then tried to arrange classroom training in occupational skills, an on-the-job training position, basic education, or other possible JTPA services.

Why would some members of the treatment group never be enrolled in JTPA? Four factors help explain why this would occur.

1. *Applicants may change their minds about JTPA* as they continue to seek other opportunities or learn more about the program. Many are looking for work on their own, and some will find employment. Some may discover different ways to finance the type of training they seek. And still others may decide that they are not interested in a job or training after all.

2. *The design of the JTPA program encourages local staff to make sure that applicants are going to participate in a service before they are enrolled* and counted as a JTPA participant. An SDA's success in exceeding its performance standards is often given great weight as a sign of how well a program is operated and up to 6 percent of Title II-A funds are incentive grants based on SDA performance. The standards' visibility within the JTPA system goes well beyond their limited role in allocating incentive grants, however. This clearly encourages SDAs to focus on achieving their standards with respect to the various outcomes measured, but it also creates an incentive to be careful whom they enroll and to hold off on enrolling individuals (that is, having them count as part of the program) until they are placed in and begin a service that SDA staff feel is likely to produce successful impacts.[1] In extreme cases, SDAs may delay enrollment in stand-alone job search assistance activities until individuals find employment.

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[1] Although the performance standard system has changed in recent years to include longer-term measures of success (that is, thirteen weeks post-program) and those less closely tied to immediate employment and low costs, the basic incentives remain unchanged. SDAs can be seen as well run and gain some additional funding if a high proportion of people leaving their program find a job that pays well or if they attain a variety of employability-enhancing competencies.

3. *Many SDAs believe that they have flexibility in defining the point at which individuals "count" in their performance standards* and responded to the incentives of the standards by delaying enrollment. In the initial years of JTPA, the Department of Labor adopted the position that JTPA was to be primarily controlled by states and localities. Therefore, the Department did not define precisely many key administrative terms, including the point at which enrollment should occur. In recent years, this federal stance has changed, but a holdover from the initial period is the continued practice of linking enrollment to the actual beginning of the intended service, be it the first day of class attended or the first workday for OJT.

4. *Despite the initial assessment that a client was appropriate for JTPA, staff may be unable to find a service provider willing to accept the person.* Service providers often retain the final say on whether they will accept an applicant. Many classroom training agencies have entrance requirements or minimum initial skills and employers who could provide OJT will typically make their own assessment about whether they want the applicant as an employee. In addition, an applicant may be seeking classroom or other training at a time of the year when it is not offered.

What Happened to Nonenrolled Assignees?

To determine this, the research team examined two questions. First, what was the typical policy concerning the point at which JTPA enrollment occurred? The finding mentioned earlier—that 64 percent of assignees were enrolled in JTPA—means that staff did not automatically enroll all members of the treatment group following random assignment. Through interviews with site staff, it was learned that most of the study sites enrolled individuals in classroom training when they attended their first class or in OJT when they worked their first day. In a few unusual sites, local staff could refer people to job search assistance or a job club without enrolling them and observe how they acted in this setting as part of an "extended assessment." The applicants referred in this manner to a job club might never be enrolled in JTPA unless they were to find a job or were referred to another activity because their behavior in the job club showed motivation and promise of employability. These findings established the possibility that staff could have worked with members of the treatment group following random assignment in an effort to arrange JTPA services without ever enrolling them.

In order to understand the extent to which this occurred, the research team drew a random sample of treatment group members in 12 sites and interviewed local staff about their efforts to work with nonenrolled assignees *after* random assignment. Twelve sites were chosen from among the 14 still conducting random assignment at the time this special study was drawn. In the remaining two sites, logistical difficulties prevented us from conducting the study and processing the data.

A ceiling of approximately 800 treatment group members was set for these sites to avoid overburdening site staff. Cases were apportioned among the sites based roughly on their proportion of treatment group members as of that date. In the case of the largest site (Fort Wayne, Indiana), the total sample was reduced from what it otherwise would have been because of concerns about staff burden. Cases were drawn during two time periods (November 1988-January 1989 and March 1989-June 1989) to lessen the influence of seasonal variation in local program practices. This might occur because the difficulty of arranging certain types of services might vary over the year. Local staff received lists of treatment group members approximately three months after they were randomly assigned. This time period was chosen to allow sufficient time for JTPA enrollment to occur (most enrollments occurred within two months of random assignment) without delaying so long that local staff would have difficulty remembering whether and how they worked with assignees following random assignment. Local staff then identified those individuals on the list who were enrolled in JTPA; these were removed from the sample. This left 307 individuals who were nonenrollees. Local staff (usually SDA staff, but in some cases service provider staff as well) were then asked to report about post-random assignment contact with nonenrollees. To help staff in this task, the list of individuals included the date on which they were randomly assigned.

Table F-1 presents the findings from this special study. The local staff had no contact with 15 percent of this sample after random assignment; basically, they were unable to locate them again. Another 11 percent of the sample reported that they were no longer interested in JTPA, for a variety of reasons. Staff were able to recontact an additional 20 percent of the sample who were still interested in JTPA, but for whom the staff never did arrange any service. The remaining 53 percent of the sample of nonenrolled treatment group members had some post-random-assignment involvement with JTPA without being enrolled. The most common activity—provided for 36 percent of the nonenrollee sample—was one or more referrals to employers for a possible OJT position. Twenty percent of the sample participated in a job club or other job search assistance. This small study suggests that local staff did work with about half of the treatment group members who were never enrolled in JTPA, though in many cases little service was provided.

Putting this together with the overall 64 percent enrollment rate in JTPA for the study sample, the results from this survey suggest that local staff worked (with a wide range of intensity and commitment to reaching an enrollment) with slightly more than 80 percent of the treatment group. This group consists of the 64 percent who were enrolled, plus an additional 18 percent (that is, half of the 36 percent who were nonenrollees) who were never enrolled but did have some post-random assignment JTPA involvement. The remaining individuals could not be recontacted after random assignment, lost interest in the program (for a variety of reasons), or were interested in the program but never received any JTPA services.

In general, the extent of JTPA services to nonenrolled assignees varied by service strategy. It was highest for those in the OJT/JSA service strategy and lowest for those in classroom training.

To summarize these findings, about half of the nonenrolled assignees in this sample received some type of JTPA services following random assignment. For those in the classroom training and OJT/JSA service strategy, these services typically were much less intensive than those received by enrollees. In most cases, the nonenrolled JTPA services consisted primarily of some additional assessment and counseling and referrals to potential service providers or on-the-job training employers. However, those referred to job search assistance may have received a service similar to others who received job search assistance as a stand-alone activity but who did find employment and were enrolled in the program. In any case, the fact that the SDA staff did not enroll these individuals in JTPA suggests that these referrals and services did not lead directly to employment.

Exhibit F.1 Distribution of Further JTPA Activity and Service Receipt after Random Assignment: A Random Sample of Treatment Group Nonenrollees in the 18-Month Study Sample

Action	*Percentage of group*
No further contact	14.7%
Contact, but not eligible	1.3
Client no longer interested	11.1
Reasons:	
Got job on own	4.6
Moved	1.6
Health problems	1.3
In another program	1.0
Uknown reason	2.6
Client interested, made contact only; received no services	20.2
Client interested; received some service	53.1
Service:	
Received further assessment and counseling	11.1
Referred to classroom training provider(s)	5.2
Provided support service	2.3
Referred to employer(s) for possible OJT	36.2
Participated in job club or job search assistance	19.9

Source: Sample of 307 treatment group members who never enrolled in JTPA.

Note: Sample members receiving some service could receive more than one type of service.

Appendix G

Estimation of Impacts on Latent Wage Rates

A program can affect earnings through either or both factors: the time spent in employment and the income people are able to earn per unit of time in employment. For those who work, hourly earnings are a measure of the latter concept.[1] Unfortunately, we do not observe any outcome for nonworkers that is comparable to hourly earnings. We define the *latent wage rate* as the hourly wage rate that a person could command if she worked. For a person who works, the latent wage rate is equal to actual hourly earnings. For a nonworker, the latent wage rate is unobserved. This appendix reports the results of an attempt to estimate impacts on latent wage rates by use of a selection modeling procedure that relies on restrictive assumptions about the determinants of latent wage rates and employment.

Model and Estimation Method

We apply a two-step method due to Heckman (1976). We make the following assumptions about the joint distribution of the latent wage rate, w_i, and employment status, m_i, within the target group or subgroup:

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[1] Hourly earnings may also be a useful indicator of productivity. Classical economic theory predicts that the wage will equal the value of the worker's marginal product. Reasons why this equality need not hold include market imperfections that cause the wage to deviate from value marginal product; costs of fringe benefits and payroll taxes paid by the employer; costs of general training borne by the worker in the form of reduced wages; and returns to specific training shared between the worker and the employer. Becker (1975, Chapter II, Section 1) gave a theoretical analysis of the last two points.

$$m_i = \begin{cases} 1 \text{ (employed) if } \boldsymbol{\alpha}'\mathbf{x}_i + u_i > 0 \\ 0 \text{ (not employed) if } \boldsymbol{\alpha}'\mathbf{x}_i + u_i \leq 0 \end{cases}$$

$$w_i = \boldsymbol{\beta}'\mathbf{x}_i + \gamma u_i + v_i$$

where $\mathbf{x}_i$ is a vector of regressors consisting of a constant, a dummy variable for assignment to the treatment group, and a set of baseline characteristics; u_i is normally distributed with zero mean and constant variance, conditional on $\mathbf{x}_i$; the expectation of v_i, conditional on $\mathbf{x}_i$ and u_i, is zero; and the vector α, the vector β, and the scalar γ are unknown parameters. In this model, the element of β that corresponds to the treatment group dummy variable is the impact of assignment to the treatment group on the latent wage rate.

If the assumptions above are true, Heckman's (1976) two-step method yields a consistent estimate of β: First, include both workers and nonworkers in the target group or subgroup in a probit regression of employment status on $\mathbf{x}_i$. Then, include all workers in a linear regression of hourly earnings on $\mathbf{x}_i$ and the inverse Mills ratio term $\phi(\hat{\boldsymbol{\alpha}}'\mathbf{x}_i)/\Phi(\hat{\boldsymbol{\alpha}}'\mathbf{x}_i)$, where $\hat{\boldsymbol{\alpha}}$ is the estimated coefficient vector from the probit regression, ϕ is the normal probability density function, and Φ is the cumulative normal distribution function. In this second-stage regression, the vector of estimated coefficients on $\mathbf{x}_i$ is the estimate of β.

Defining w_i to be the ratio of total earnings to total hours worked during the fifth and sixth quarters after random assignment and m_i to equal 1 if person i was employed at all in those quarters and 0 if not, we used this two-step method to estimate impacts on w_i. We estimated separate regressions for each target group and for each service strategy subgroup within each target group.[2] The regressors $\mathbf{x}_i$ were those listed in Appendix D, Section 4 (omitting certain variables in the subgroup regressions to avoid multicollinearity).

The estimated coefficient on treatment in the second-stage regression is our estimate of the impact of assignment to the treatment group on latent wage rates. We estimate the control mean by substituting the target group or subgroup mean covariate values into the estimated latent wage equation and setting the treatment group dummy variable and the inverse Mills ratio term to zero.

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[2] We used data on First Follow-up Survey respondents only. Unlike the estimates of impact on the earnings and employment of adult women, the estimates of impact on the latent wage rates of adult women are not derived from a nonresponse bias adjustment involving Unemployment Insurance (UI) records, because the UI data do not measure hours worked.

Our significance tests use conventional estimates of standard errors from the second-stage regressions. Because these standard error estimates are not generally consistent, we also computed the White (1980) estimates, which are consistent (Amemiya 1985, pp. 370, 387), for several of the wage regressions. The conventional and White standard error estimates did not differ appreciably.

Note that because the employment and latent wage equations in our model contain the same regressors, the inverse Mills ratio term in the second-stage regression is simply a nonlinear transformation of the other regressors, including treatment. (It is difficult to make a convincing argument that a regressor in one equation neither influences the other outcome nor is correlated with omitted variables that influence the other outcome.) The model assumptions of linearity, normality, and homoskedasticity are essential to the reliability of the procedure. Goldberger (1983) and others have shown that under plausible departures from these assumptions, estimators of this type can have substantial bias.

Results

Exhibit G.1 shows the estimated control mean and impact for each target group and for each service strategy subgroup within each target group. None of the estimated impacts are statistically significant at the .10 level.

In light of the known fragility of the procedure under departures from the model assumptions, the results cannot be regarded as definitive. Although it is possible to test a subset of the model assumptions (e.g., Andrews 1988) or to apply methods that rely on less restrictive assumptions (e.g., Newey, Powell, and Walker 1990), no econometric procedure can provide definitive estimates of impacts on latent wage rates without information external to our data, given that the outcome is unobserved for the substantial fraction of the sample that was not employed.

Exhibit G.1 *Estimated Impacts on Latent Hourly Wage Rates in the Fifth and Sixth Quarters after Random Assignment: JTPA Assignees in the 18-Month Study Sample, by Target Group and Service Strategy Subgroup*

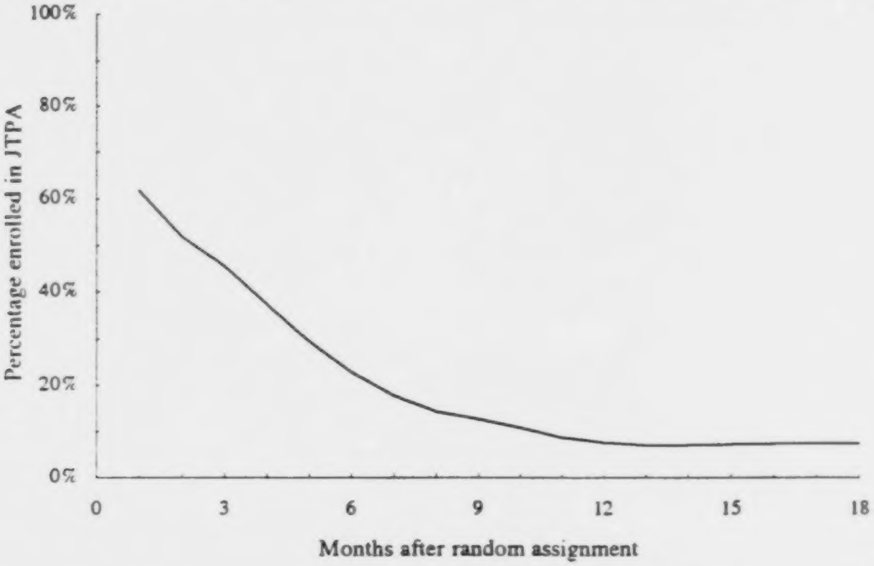
Target group/ subgroup	*Sample size (workers and nonworkers) (1)*	*Sample size (workers only) (2)*	*Control mean (3)*	*Impact, in $ (4)*	*Standard error (5)*
Adult women	5,724	3,912	$ 5.55	$ 0.14	0.10
Classroom training	2,529	1,606	6.90	0.18	0.17
OJT/JSA	2,048	1,521	5.45	0.14	0.13
Other services	1,147	785	5.82	-0.05	0.18
Adult men	4,419	3,374	6.68	0.00	0.11
Classroom training	1,057	807	6.88	-0.12	0.23
OJT/JSA	2,250	1,750	5.72	0.15	0.15
Other services	1,112	817	6.67	-0.17	0.22
Female youths	2,300	1,509	4.91	0.05	0.11
Classroom training	1,045	666	4.79	0.19	0.16
OJT/JSA	545	397	5.29	-0.15	0.32
Other services	710	446	4.74	0.04	0.19
Male youths	1,748	1,420	5.74	-0.01	0.14
Classroom training	526	439	5.56	0.03	0.21
OJT/JSA	615	525	5.92	0.09	0.29
Other services	607	456	5.72	-0.19	0.22

Source: Estimates based on First Follow-up Survey responses.
Note: None of the estimated impacts (column 4) were statistically significant at the .10 level.

Appendix H

Supplementary Exhibits on Out-of-School Youth

Exhibit H.1 Percentage Enrolled in JTPA Monthly: Female Youth Treatment Group



Source: Unadjusted frequencies based on enrollment and tracking data from the 16 service delivery areas (SDAs).
Note: Sample size, treatment group = 1,586.

Exhibit H.2 Impacts on the Distribution of Total 18-Month Earnings: Female Youth JTPA Assignees

18-month earnings	*Assignees (1)*	*Control group (2)*	*Difference, in % pts. (3)*
$0	18.9%	22.4%	-3.5%**
$1 - $2,537	21.5	19.5	2.0
$2,538 - $6,593	21.9	19.3	2.6
$6,594 - $11,762	18.7	19.3	-0.6
> $11,762	19.0	19.5	-0.5
Chi-squared test of impact on entire distribution			not significant

Source: Estimates based on First Follow-up Survey responses.
Notes: Sample size, assignees =1,586; control group =714. For the estimation procedure, see Appendix D.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (chi-squared test or two-tailed t-test).

Exhibit H.3 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: Female Youth JTPA Assignees and Enrollees.*

Period	Control mean (1)	*Impact per assignee* In % pts., weeks, or hours (2)	*Impact per assignee* As % of (1) (3)	Inferred impact per enrollee, in % pts., weeks, or hours (4)
		Percentage employed		
Quarter 1	44.7%	-0.6%	-1.2%	-0.9%
2	51.7	-2.3	-4.5	-3.8
3	54.2	-0.5	-1.0	-0.9
4	55.3	0.8	1.4	1.3
5	57.0	0.1	0.1	0.1
6	59.3	-1.0	-1.7	-1.7
Anytime during quarters 1 - 6	78.1	2.8	3.6	4.5
		Weeks worked		
Quarter 1	4.8	-0.4*	-8.5%	-0.7
2	5.5	-0.6**	-10.4	-1.0
3	5.9	-0.4	-6.1	-0.6
4	5.9	0.0	0.4	0.0
5	6.1	0.0	0.6	0.1
6	6.4	-0.3	-4.8	-0.5
All quarters	34.7	-1.6	-4.6	-2.6
		Hours worked		
Quarter 1	174	-16*	-9.0%	-25
2	206	-19*	-9.0	-30
3	230	-18*	-8.0	-30
4	223	1	0.4	1
5	229	4	1.6	6
6	239	-13	-5.4	-21
All quarters	1,302	-61	-4.7	-99

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 1,586; control group = 714. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

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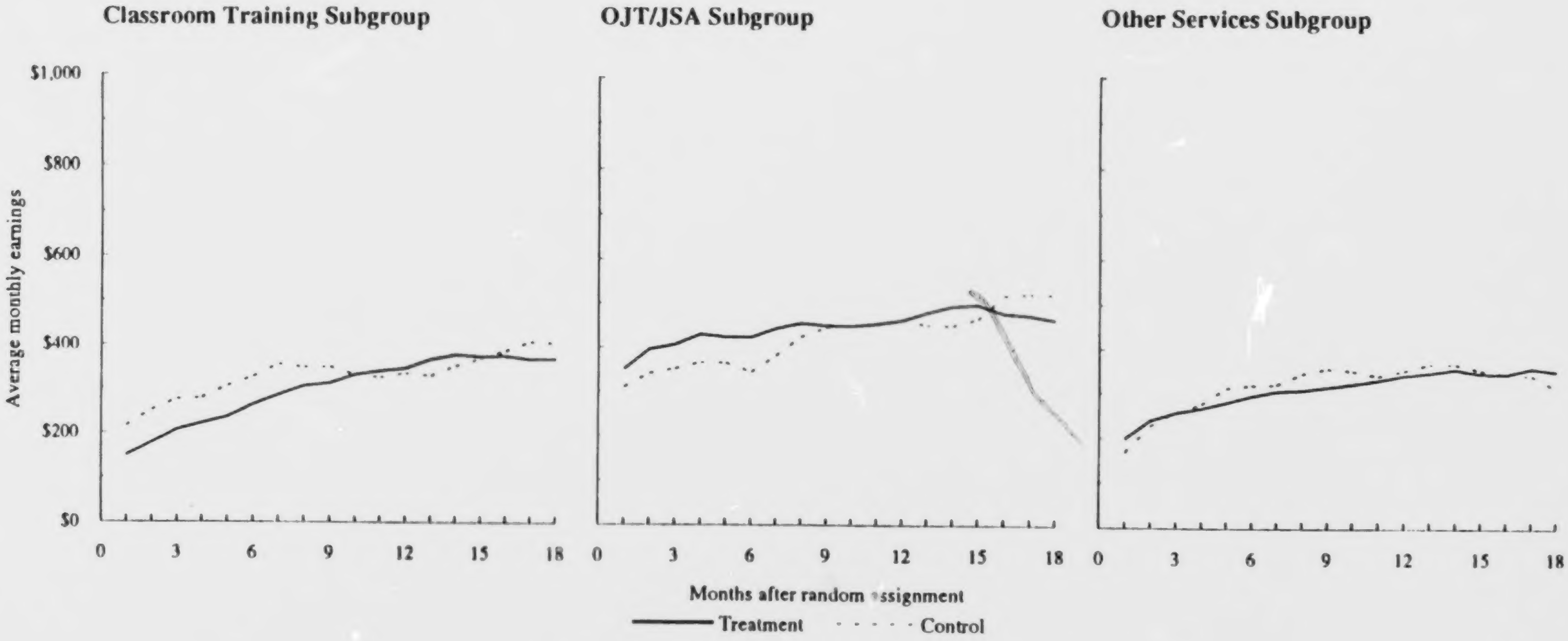
Exhibit H.4 Percentage Impacts on Earnings and Its Components: Female Youth JTPA Assignees

Period		*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
Quarter	1	-6.4%	-1.2%	-7.4%	-0.7%	3.0%
	2	-5.9	-4.5	-6.2	1.5	3.4
	3	-6.8	-1.0	-5.2	-2.0	1.3
	4	0.2	1.4	-0.9	-0.1	-0.2
	5	4.4	0.1	0.3	1.1	2.8
	6	-4.5	-1.7	-3.0	-0.7	1.0
All quarters		-2.9	3.6	-7.9	-0.1	1.9

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 1,586; control group = 714. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is displayed as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

Exhibit H.5 Average Monthly Earnings of Female Out-of-School Youths: Treatment Group and Control Group, by Service Strategy Subgroup



Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: treatment group =704, control group = 341; OJT/JSA subgroup: treatment group = 381, control group = 164; other services subgroup: treatment group = 501, control group = 209. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

Exhibit H.6 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: Female Youth JTPA Assignees, by Service Strategy Subgroup*

	Percentage employed		*Weeks worked*		*Hours worked*	
Period	*Control mean (1)*	*Impact, in % pts. (2)*	*Control mean (3)*	*Impact, in weeks (4)*	*Control mean (5)*	*Impact, in hours (6)*
			Classroom training subgroup			
Quarter 1	44.2	-8.7%***	4.6	-1.2***	164	-46***
2	50.9	-6.6**	5.3	-1.1***	197	-44***
3	49.9	1.4	5.7	-0.5	221	-30**
4	49.9	4.1	5.4	0.3	206	1
5	54.8	0.9	5.8	0.2	214	8
6	57.1	0.7	6.2	-0.1	228	-8
All quarters [a]	75.2	3.5	32.8	-2.4	1,231	-119*
			OJT/JSA subgroup			
Quarter 1	53.9	6.0%	5.9	0.3	228	16
2	55.1	5.7	6.2	0.3	238	21
3	61.4	1.4	6.7	0.0	263	6
4	64.1	-1.2	7.1	-0.5	275	-7
5	67.0	-3.5	7.3	-0.3	282	-2
6	70.1	-6.3	8.1	-1.3**	308	-47**
All quarters [a]	84.8	3.6	41.3	-1.4	1,593	-13
			Other services subgroup			
Quarter 1	37.4	7.2%*	4.0	0.4	142	11
2	50.3	-2.4	5.3	-0.5	194	-11
3	54.3	-3.7	5.7	-0.4	215	-18
4	55.5	-1.0	5.7	0.1	209	6
5	53.4	0.6	5.8	0.0	213	1
6	54.1	0.6	5.7	0.1	201	7
All quarters [a]	76.2	2.6	32.1	-0.4	1,175	-4

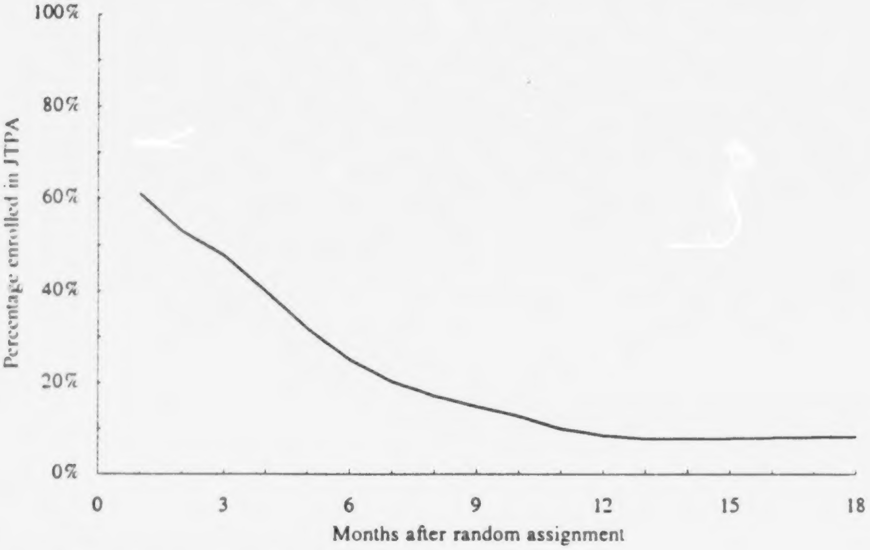
Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 704, control group = 341; OJT/JSA subgroup, assignees = 381, control group = 164; other services subgroup: assignees = 501, control group = 209. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

[a] For columns 1 and 2 ("percentage employed") this row shows the percentage of control group members who reported being employed at any time during the follow-up period and the estimated impact on this percentage for assignees, respectively.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Exhibit H.7 Percentage Enrolled in JTPA Monthly: Male Youth Treatment Group



Source: Unadjusted frequencies based on enrollment and tracking data from the 16 service delivery areas (SDAs).
Note: Sample size, treatment group = 1,196.

Exhibit H.8 Impacts on the Distribution of Total 18-Month Earnings: Male Youth JTPA Assignees

18-month earnings	*Assignees* *(1)*	*Control group* *(2)*	*Difference, in % pts.* *(3)*
$0	9.5%	10.9%	-1.4%
$1 - $5,236	27.1	22.3	4.8**
$5,237 - $10,210	21.1	22.3	-1.2
$10,211 - $17,222	24.2	22.3	1.9
> $17,222	18.1	22.3	-4.2**
Chi-squared test of impact on entire distribution			not significant

Source: Estimates based on First Follow-up Survey responses.
Notes: Sample size, assignees = 1,196; control group = 552. For the estimation procedure, see Appendix D.
* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (chi-squared test or two-tailed t-test).

Exhibit H.9 Impacts on the Percentage Employed and on the Mean Number of and Hours Worked: Male Youth JTPA Assignees and Enrollees

		Impact per assignee		
Period	*Control mean (1)*	*In % pts., weeks, or hours (2)*	*As % of (1) (3)*	*Inferred impact per enrollee, in % pts., weeks, or hours (4)*
		Percentage employed		
Quarter 1	58.1%	-3.4%	-5.8%	-5.4%
2	63.1	-0.5	-0.8	-0.9
3	68.8	-2.1	-3.0	-3.3
4	71.3	-3.3	-4.6	-5.3
5	73.6	-1.6	-2.2	-2.7
6	77.5	-2.7	-3.6	-4.4
Anytime during quarters 1 - 6	89.0	1.5	1.7	2.42.4
		Weeks worked		
Quarter 1	6.4	-0.6**	-9.4%	-1.0
2	7.0	-0.2	-3.1	-0.4
3	7.7	-0.5	-5.9	-0.7
4	7.9	-0.4	-5.1	-0.7
5	8.2	-0.1	-1.7	-0.2
6	8.5	-0.4	-5.1	-0.7
All quarters	45.7	-2.3*	-4.9	-3.6
		Hours worked		
Quarter 1	257	-30**	-11.8%	-48
2	287	-9	-3.3	-15
3	326	-27**	-8.3	-43
4	333	-24*	-7.2	-38
5	343	-14	-4.0	-22
6	346	-24*	-7.0	-39
All quarters	1,892	-129**	-6.8	-205

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 1,196; control group = 552. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

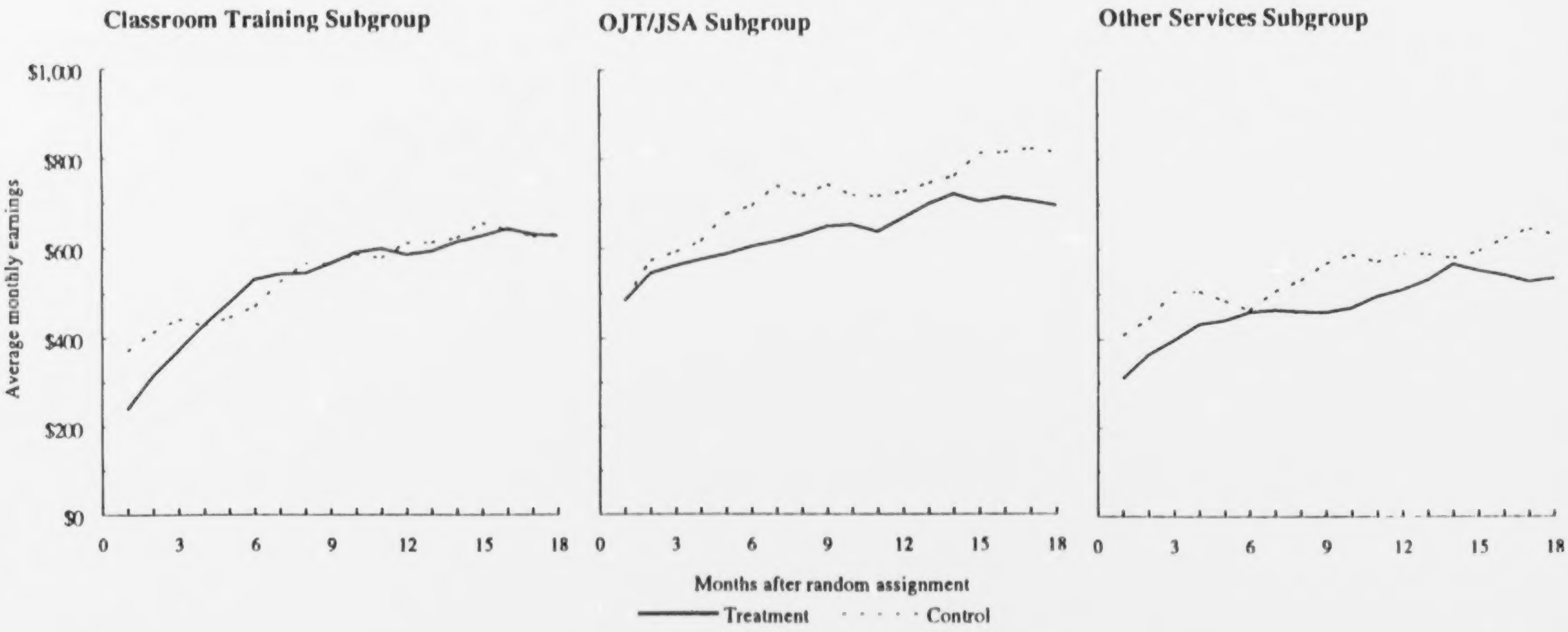
Exhibit H.10 Percentage Impacts on Earnings and Its Components: Male Youth JTPA Assignees

Period		*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
Quarter	1	-14.1%	-5.8%	-4.0%	-2.6%	-2.6%
	2	-4.5	-0.8	-2.2	-0.2	-1.2
	3	-8.4	-3.0	-2.9	-2.6	-0.1
	4	-7.3	-4.6	-0.4	-2.3	-0.1
	5	-5.3	-2.2	0.6	-2.3	-1.3
	6	-9.2	-3.6	-1.5	-2.0	-2.3
All quarters		-7.9	1.7	-6.5	-2.0	-1.2

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 1,196; control group = 552. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is displayed as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

Exhibit H.11 Average Monthly Earnings of Male Out-of-School Youths: Treatment Group and Control Group, by Service Strategy Subgroup



Source: Estimates based on First Follow-up Survey responses.
Notes: Sample size, classroom training subgroup: treatment group =354, control group = 172; OJT/JSA subgroup: treatment group = 411, control group = 204; other services subgroup: treatment group = 431, control group = 176. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

Exhibit H.12 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: Male Youth JTPA Assignees, by Service Strategy Subgroup*

	Percentage employed		*Weeks worked*		*Hours worked*	
Period	*Control mean (1)*	*Impact, in % pts. (2)*	*Control mean (3)*	*Impact, in weeks (4)*	*Control mean (5)*	*Impact, in hours (6)*
			Classroom training subgroup			
Quarter 1	54.8	-6.3%	6.1	-1.2**	228	-54**
2	58.2	3.6	6.4	0.1	243	13
3	66.3	2.6	7.3	0.1	293	3
4	68.8	0.3	7.7	0.0	309	2
5	72.2	-0.2	8.0	0.0	326	-9
6	78.2	-1.8	8.3	-0.2	327	-7
All quarters [a]	89.1	2.4	43.8	-1.3	1,727	-50
			OJT/JSA subgroup			
Quarter 1	63.0	1.5%	6.8	0.3	289	6
2	71.8	-4.0	8.1	-0.5	347	-24
3	76.8	-5.0	8.8	-0.8	391	-53**
4	77.0	-5.5	8.4	-0.3	377	-36
5	79.0	-2.4	9.1	-0.3	394	-32
6	83.1	-4.4	9.3	-0.7	393	-44*
All quarters [a]	92.5	0.9	50.4	-2.3	2,190	-182
			Other services subgroup			
Quarter 1	57.5	-7.7%*	6.2	-1.0*	254	-52**
2	58.1	-0.2	6.4	-0.2	267	-18
3	62.1	-2.1	7.0	-0.6	293	-34
4	67.8	-4.4	7.6	-0.9*	315	-43*
5	69.4	-2.4	7.4	0.1	305	3
6	69.9	-0.3	7.9	-0.5	318	-25
All quarters [a]	85.2	1.6	42.4	-3.0	1,752	-169

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 354, control group = 172; OJT/JSA subgroup, assignees = 411, control group = 204; other services subgroup: assignees = 431, control group = 176. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

[a] For columns 1 and 2 ("percentage employed") this row shows the percentage of control group members who reported being employed at any time during the follow-up period and the estimated impact on this percentage for assignees, respectively.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

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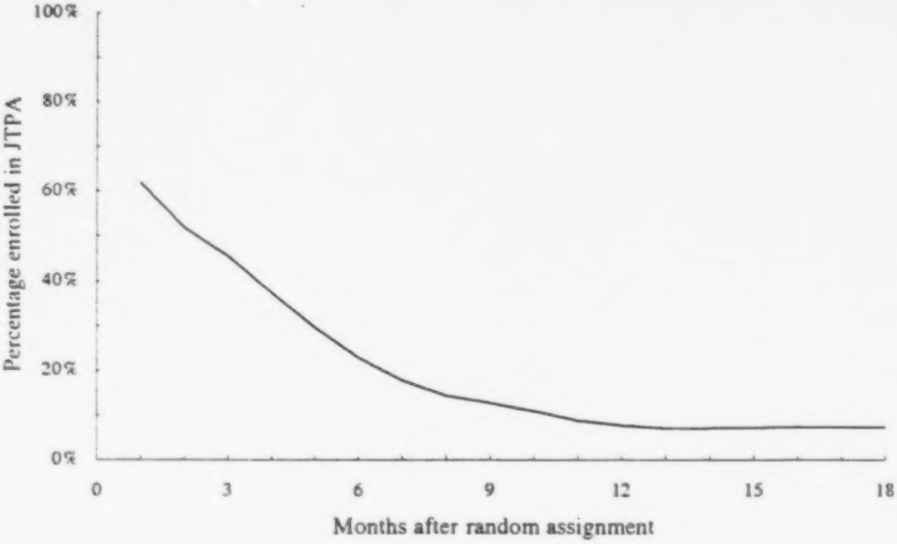
Exhibit H.13 Percentage Enrolled in JTPA Monthly: Treatment Group, All Youths



Source: Unadjusted frequencies based on enrollment and tracking data from the 16 service delivery areas (SDAs).
Note: Sample size, treatment group = 2,782.

Exhibit H.14 Impacts on Earnings: All Youth JTPA Assignees and Enrollees

Period		Control mean (1)	*Impact per assignee*		Inferred impact per enrollee, in $ (4)
			In $ (2)	*As % of (1)* (3)	
Quarter	1	$ 1,058	$ -113***	-10.7%	$ -182
	2	1,232	-57	-4.6	-91
	3	1,399	-97**	-6.9	-155
	4	1,430	-48	-3.4	-77
	5	1,501	-8	-0.5	-13
	6	1,587	-104**	-6.5	-166
All quarters		8,207	-427*	-5.2	-684

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 2,782; control group = 1,266. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).
Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

Exhibit H.15 Impacts on the Distribution of Total 18-Month Earnings: All Youth JTPA Assignees

18-month earnings	Assignees (1)	Control group (2)	Difference, in % pts. (3)
$0	14.8%	17.4%	-2.6%**
$1 - $3,598	24.1	20.7	3.4**
$3,599 - $8,248	22.3	20.6	1.7
$8,249 - $14,178	19.8	20.6	-0.8
> $14,178	18.9	20.7	-1.8
Chi-squared test of impact on entire distribution			**

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 2,782; control group = 1,266. For the estimation procedure, see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (chi-squared test or two-tailed t-test).

Exhibit H.16 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: All Youth JTPA Assignees and Enrollees*

		Impact per assignee		
Period	*Control mean (1)*	*In % pts., weeks, or hours (2)*	*As % of (1) (3)*	*Inferred impact per enrollee, in % pts., weeks, or hours (4)*
		Percentage employed		
Quarter 1	50.4%	-1.6%	-3.1%	-2.5%
2	56.4	-1.2	-2.2	-2.0
3	60.3	-0.9	-1.5	-1.5
4	61.9	-0.5	-0.9	-0.9
5	64.0	-0.4	-0.7	-0.7
6	67.0	-1.5	-2.3	-2.5
Anytime during quarters 1 - 6	82.7	2.4**	2.9	3.9
		Weeks worked		
Quarter 1	5.4	-0.5**	-8.6%	-0.8
2	6.2	-0.4**	-6.3	-0.6
3	6.7	-0.4*	-5.4	-0.6
4	6.7	-0.1	-1.5	-0.2
5	7.0	0.0	0.0	0.0
6	7.3	-0.3*	-4.4	-0.5
All quarters	39.2	-1.6*	-4.2	-2.7
		Hours worked		
Quarter 1	209	-21***	-10.1%	-34
2	240	-13	-5.4	-21
3	270	-20**	-7.5	-33
4	270	-8	-3.1	-14
5	277	-2	-0.7	-3
6	284	-15*	-5.5	-25
All quarters	1,550	-80**	-5.2	-131

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 2,782; control group = 1,266. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

Exhibit H.17 Percentage Impacts on Earnings and Its Components: All Youth JTPA Assignees

Period	*Earnings per assignee (1)*	*Workers per assignee (2)*	*Weeks worked per worker (3)*	*Hours worked per week worked (4)*	*Earnings per hour worked (5)*
Quarter 1	-10.7%	-3.1%	-5.5%	-1.7%	-0.7%
2	-4.6	-2.2	-4.1	1.1	0.8
3	-6.9	-1.5	-4.0	-2.2	0.6
4	-3.4	-0.9	-0.8	-1.5	-0.3
5	-0.5	-0.7	0.5	-0.6	0.2
6	-6.5	-2.3	-2.1	-1.1	-1.2
All quarters	-5.2	2.9	-6.9	-1.1	0.0

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, assignees = 2,782; control group = 1,266. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

Columns 2 through 5 display the impact as a percentage of the corresponding control mean (not shown). For column 2 this means the impact on the employment rate is displayed as a percentage of the mean rate for the control group. Tests of statistical significance were not performed for any of the columns in this exhibit.

Exhibit H.18 *Impacts on Attainment of a Training-Related High School Diploma or GED Certificate: All Youth JTPA Assignees Overall and High School Dropout Subgroup, by Service Strategy Subgroup*

	Sample size[a] *(1)*	*Percentage attaining a training-related high school credential*[b] *Assignees (2)*	*Control group (3)*	*Difference, in % pts. (4)*
		Classroom training subgroup		
Full sample	1,511	15.8%	8.5%	7.3%***
High school dropouts	769	30.7	17.2	13.5***
		OJT/JSA subgroup		
Full sample	1,111	4.7	1.9	2.7***
High school dropouts	408	12.7	5.3	7.4***
		Other services subgroup		
Full sample	1,267	18.8	12.5	6.4***
High school dropouts	828	28.8	19.0	9.8***
		All subgroups		
Full sample	3,889	13.7	7.8	5.9***
High school dropouts	2,005	26.3	15.4	10.9***

Sources: Unadjusted frequencies based on Background Information Form responses and First Follow-up Survey responses.

[a] Treatment and control groups combined.

[b] "Attainment of a training-related high school credential" is defined as the combination of having received some school or training service and having attained a high school diploma or General Educational Development certificate at some time during the 18-month follow-up period.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Exhibit H.19 *Impacts on Earnings: All Youth JTPA Assignees and Enrollees, by Service Strategy Subgroup*

Period	Control mean (1)	Impact per assignee		Inferred impact per enrollee, in $ (4)
		In $ (2)	As % of (1) (3)	
		Classroom training subgroup		
Quarter 1	$ 916	$ -250***	-27.3%	$ -366
2	1,063	-92	-8.6	-135
3	1,262	-98	-7.7	-143
4	1,267	10	0.8	15
5	1,339	31	2.3	45
6	1,434	-44	-3.1	-64
All quarters	7,281	-443	-6.1	-648
		OJT/JSA subgroup		
Quarter 1	$ 1,351	$ 39	2.8%	$ 72
2	1,566	-18	-1.1	-33
3	1,750	-102	-5.8	-188
4	1,778	-93	-5.2	-171
5	1,869	-39	-2.1	-72
6	2,019	-222**	-11.0	-408
All quarters	10,333	-435	-4.2	-800
		Other services subgroup		
Quarter 1	$ 992	$ -107	-10.7%	$ -172
2	1,166	-84	-7.2	-135
3	1,282	-129	-10.1	-208
4	1,375	-149*	-10.9	-240
5	1,414	-77	-5.4	-124
6	1,423	-104	-7.3	-168
All quarters	7,652	-650	-8.5	-1,048

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 1,058, control group = 513; OJT/JSA subgroup: assignees = 792, control group = 368; other services subgroup: assignees = 932, control group = 385. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test). Significance levels for column 3 are identical to those in column 2. Tests of statistical significance were not performed for column 4.

Exhibit H.20 *Impacts on the Percentage Employed and on the Mean Number of Weeks and Hours Worked: All Youth JTPA Assignees, by Service Strategy Subgroup*

	Percentage employed		*Weeks worked*		*Hours worked*	
	Control mean	*Impact, in % pts.*	*Control mean*	*Impact, in weeks*	*Control mean*	*Impact, in hours*
Period	*(1)*	*(2)*	*(3)*	*(4)*	*(5)*	*(6)*
			Classroom training subgroup			
Quarter 1	47.9	-8.0%***	5.1	-1.2***	186	-49***
2	53.1	-2.8	5.7	-0.7**	212	-24**
3	55.7	1.3	6.2	-0.3	246	-20
4	56.4	2.6	6.2	0.1	243	-2
5	60.4	0.8	6.5	0.1	252	1
6	64.2	-0.1	6.9	-0.1	261	-7
All quarters[a]	79.8	3.2	36.6	-2.1	1,400	-102*
			OJT/JSA subgroup			
Quarter 1	58.8	3.7%	6.4	0.3	261	10
2	64.4	-0.1	7.2	-0.1	296	-2
3	69.7	-2.1	7.8	-0.4	330	-23
4	70.8	-3.2	7.7	-0.3	326	-18
5	72.7	-1.9	8.2	-0.2	338	-13
6	76.7	-4.9*	8.6	-0.8**	349	-39**
All quarters[a]	89.0	2.1	45.9	-1.5	1,900	-86
			Other services subgroup			
Quarter 1	46.4	0.7%	5.0	-0.2	193	-17
2	53.7	-1.1	5.8	-0.3	226	-12
3	57.8	-2.8	6.2	-0.4	248	-22
4	61.2	-2.6	6.6	-0.4	258	-16
5	61.2	-1.5	6.6	-0.1	257	0
6	61.7	-0.2	6.7	-0.2	256	-9
All quarters[a]	80.4	2.0	36.9	-1.6	1,437	-75

Source: Estimates based on First Follow-up Survey responses.

Notes: Sample size, classroom training subgroup: assignees = 1,058, control group = 513; OJT/JSA subgroup: assignees = 792, control group = 368; other services subgroup: assignees = 932, control group = 385. Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

[a] For columns 1 and 2 ("percentage employed") this row shows the percentage of control group members who reported being employed at any time during the follow-up period and the estimated impact on this percentage for assignees, respectively.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

Exhibit H.21 *Impacts on Total 18-Month Earnings: All Youth JTPA Assignees, by Gender and Ethnicity*

Subgroup	Sample size (1)	Control mean (2)	Impact, in $ (3)	Impact, in $, adjusted for sample distribution across: Sites (4)	Impact, in $, adjusted for sample distribution across: Sites and service strategies (5)
Female	2,300	$ 6,202	$ -163	$ -236	$ -327
Male	1,748	10,799	-773**	-840**	-939**
F-test, difference between genders			n.s.	n.s.	n.s.
Whites	2,094	$ 9,628	$ -607*	$-892**	$ -1,036**
Nonwhites [a]	1,954	6,776	-238	81	47
F-test, difference between genders			n.s.	n.s.	*

Source: Estimates based on First Follow-up Survey responses.

Notes: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D. Control group means are not regression-adjusted.

a. This subgroup includes blacks, Hispanics, American Indians, Alaskan Natives, Asians, and Pacific Islanders.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (F-test or two-tailed t-test); "n.s." means the F-test for the difference in impacts between the subgroups in each set is not statistically significant.

Exhibit H.22 *Deviations of Impacts on the Total 18-Month Earnings of Each Two-Year Youth Cohort from the Trend in Impacts on the 18-Month Earnings of Young Adults Ages 22 to 29: JTPA Assignees, by Gender and Service Strategy Subgroup*

Service strategy subgroup	Sample size[a] (1)	Deviation of impact on 2-year youth cohort from trend in young adult impacts (in $) [b]: Age 16-17 (2)	Age 18-19 (3)	Age 20-21 (4)
		Females		
Classroom training	2,221	$ 2,153	$ 782	$ 738
OJT/JSA	1,394	-1,790	-1,021	118
Other services	1,152	-643	-441	-2,884
All subgroups	4,767	933	211	-169
		Males		
Classroom training	1,050	$ -3,692	$ -3,995	$ -1,075
OJT/JSA	1,608	-1,871	-3,624	-1,299
Other services	1,049	194	1,806	677
All subgroups	3,707	-1,912	-2,188	-702

Source: Estimates based on First Follow-up Survey responses.

Note: Estimates are regression-adjusted to control for differences in baseline characteristics between the treatment group and control group; see Appendix D.

a. Treatment and control groups combined, including all sample members who were ages 16 to 29 at random assignment.

b. None of the deviations in columns 2 through 4 was statistically significant at the .10 level (two-tailed test).

Exhibit H.23 *Impacts on the Number of Months of Continuous Nonemployment between Random Assignment and the First Job after Random Assignment: Youth Treatment Group and Control Group, by Gender and Service Strategy Subgroup*

		Month of first job		*Impact on nonemployment*	
Service strategy subgroup	*Sample size* [a] *(1)*	*Treatment group (2)*	*Control group (3)*	*In months (4)*	*As % of (3) (5)*
			Female youths		
Classroom training	1,045	5.5	4.6	0.9**	19.0%
OJT/JSA	545	3.3	4.3	-1.1**	-24.9
Other services	710	4.5	4.8	-0.3	-6.1
All subgroups	2,300	4.6	4.6	0.0	0.1
			Male youths		
Classroom training	526	4.6	4.3	0.3	8.0%
OJT/JSA	615	3.4	3.1	0.3	9.8
Other services	607	4.4	3.7	0.7	18.6
All subgroups	1,748	4.1	3.7	0.5*	12.5

Source: Estimates based on First Follow-up Survey responses.

Note: For the estimation procedure see Appendix D.

a. Treatment and control groups combined.

* Statistically significant at the .10 level, ** at the .05 level, *** at the .01 level (two-tailed test).

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